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THE

Psychological Review

EDITED BY

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HOWARD C. WARREN, Princeton University (*Index*)

JAMES R. ANGELL, University of Chicago (*Monographs*) and

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VOL. XXI. No. 6 November, 1914

THE PSYCHOLOGICAL REVIEW

A SCHEMA OF METHOD[1]

BY CHRISTIAN A. RUCKMICH

University of Illinois, Urbana, Illinois

This paper owes its origin to a study of the historical development of psychological methods to which the writer recently turned his attention. In the course of the investigation, it was soon evident that historically, as well as latterly, the term *method* had assumed a variety of connotations in the literature of the science. An attempt was made, therefore, to determine exactly what these connotations were and to group them under the proper rubrics. The problem, then, changed from a survey of method in general to a classification of the varieties of method. Accordingly a study of the interpretation of psychological methods as outlined in more than a score of treatises was begun. The present article is the result. Its purpose is not to offer an exhaustive logical critique of the concept,[2] but to specify the several usages of the term which were actually found in the literature. After this systematic study has been made, obviously a more adequate review of the historical development of *method* in its several aspects can then be begun.[3]

[1] The article presents, in a revised form and with greater regard for detail, the essential points advanced in the introductory section of a paper which was read before the American Psychological Association, December 30, 1913.

[2] For discussions of this order, the reader is referred to the standard works on methodology and logic, *e. g.*, Jevons, W. S., 'Principles of Science,' 3d ed., London, 1877; Pearson, K., 'Grammar of Science,' 3d ed., London, 1911; Sigwart, C., 'Logic,' 2d ed. (trans. Dendy), London, 1895; Wundt, W., 'Logik,' Vol. 3, 3d ed., Stuttgart, 1908; and 'Encyclopedia of the Philosophical Sciences' (ed. Windelband and Ruge), Vol. 1, 'Logic,' (trans. Meyer), London, 1913.

[3] It is the intention of the writer to publish in the near future a study of this development in terms of the present classification.

If we examine such phrases as *introspective method*, *statistical method*, *adjustment method*, *method of minimal changes*, *inductive method*, *genetic method*, it is clear that the word *method* is not used unequivocally.[1] We find, nevertheless, that in many text-books and systematic treatises these phrases are used coördinately under discussions of *method*. The same thing happens, moreover, in other disciplines. In a recent discussion of the methods of the physical sciences, *mathematical method*, *dynamic method*, *method of thermodynamics*, and *method of analogy*, are treated on a common plane: in a logical outline of the article, faithfully abstracted, these terms would appear as coördinate rubrics.[2] In a standard work on education there are several similar occurrences: *Scientific method*, *schoolroom method*, *psychological method*, and *sociological method* appear on equal terms in the same sentence.[3]

In addition to the fact that the term *method* is frequently used in more than one strict sense, several words are often used in the same contexts with differences of meaning which are difficult to analyse. Examples of the use of the terms *method*, *procedure*, and *instrument*, in this way, are:[4]

> The methods of psychology are, in general, the two methods of every science: description (that is, analysis and classification) and explanation. But besides these

[1] Similar examples can be found in such works as Baldwin's 'Handbook of Psychology,' Vol. 1, New York, 1890, 22ff.; James's 'Principles of Psychology,' Vol. 1, New York, 1890, 185ff.; Külpe's 'Outlines of Psychology' (trans. Titchener), London and New York, 1895, 8ff.; Ladd's 'Elements of Physiological Psychology,' New York, 1887, 6ff.; and in an address on 'Psychological Methods' by W. McDougall, published in 'Lectures on the Method of Science,' Oxford, 1906, 113ff.

[2] Webster, A. G., 'The Methods of the Physical Sciences. To What Are They Applicable?' *Science*, N.S., 39, 1914, 42–51.

[3] Monroe, P., 'A Text-book in the History of Education,' New York, 1911, 757.

[4] In many instances we find the juxtaposition of *method* and *procedure* with meanings which are respectively supplementary, indicating a recognizable distinction:

> In its effort to establish itself upon a scientific basis physiological psychology has no choice but to follow essentially the same method of procedure. Ladd, G. T., 'Elements of Physiological Psychology,' New York, 1887, 6.
>
> The nature of the investigation and method of procedure [section heading]. Swift, E. J., in 'Studies in Philosophy and Psychology,' Garman Memorial Volume, Boston and New York, 1906, 297.
>
> In every case, my plan has been to sketch the development of the test, to prescribe a standard form of apparatus and method of procedure, to explain the treatment of the data secured, and to set forth the results and conclusions thus far obtained. Whipple, G. M., 'Manual of Mental and Physical Tests,' Baltimore, 1910, vii.

fundamental forms of procedure, every science has certain methods peculiar to itself: and the method which distinguishes psychology is that of introspection.[1]

The blunder of the critics of 'introspection' lies in assuming that the results gained in a particular procedure for a particular purpose could be supposed to represent the whole state of affairs. . . . Thus on the question of the validity of introspection we have granted to both parties the main contention—namely, that it is a valid method, and that it is in a degree erroneous.[2]

In psychology the modern transformation comes most strongly out. Here we find an actual department of knowledge handed over to a new class of men for treatment, so remarkable is the demand for scientific method. It is no longer sufficient that a psychologist should be familiar with philosophy and its history, or capable of acute logical criticism of systems; it is necessary, if he would deal successfully with the new problems and gain the ear of the advanced philosophical public, that he should reason from a basis of fact and by an inductive procedure.[3]

One of the methods consists in remarking the disturbances in mental life which occur under conditions of disease and which throw into relief one or another property of our mental equipment. Such a procedure brings to notice the difference between sensation and intellect, the distinction of emotion from either, and the differentiation of volition from all three.[4]

It was to be expected, then, that there would be a longing for some mode of investigation wider in its application and more fruitful than self-observation, and that in due time there would be an organized revolt in favor of 'objective' methods, among which the experimental procedure was to have an important place.[5]

But introspection is not only a method of psychological investigation, it is also a mental fact, and as such it must be capable of psychological analysis and investigation. . . . The second problem is: how far into the nature of the various other mental processes can such an instrument be expected to penetrate.[6]

Nor is this difficulty of analysis confined characteristically to our own language. *Methode*, *Verfahren*, and *Hilfsmittel* occur in contexts in which it is not easy to find logical warrant for a distinctive terminology:[7]

These points of view have to be reckoned with in a discussion of *self-observation* as a psychological method [Methode]. Volkelt forthwith admits that when the intention to carry out the self-observation is presupposed in any individual case, self-observation becomes impossible. If one says on every occasion: Now I will observe, then this purpose will be thwarted at once. He seems to believe, however, that such a procedure [Verfahren] was hardly ever intended.[8]

[1] Calkins, M. W., 'A First Book in Psychology,' New York, 1911, 6.

[2] Scripture, E. W., 'The New Psychology,' New York, 1905, 12.

[3] Baldwin, J. M., 'Psychology Past and Present, PSYCHOL. REV., 1, 1894, 373.

[4] Angell, J. R., 'Chapters from Modern Psychology,' New York, 1913, 8f.

[5] Stratton, J. M., 'Experimental Psychology,' New York, 1903, 4.

[6] Dodge, R., 'The Theory and Limitations of Introspection, *Am. J. of Psychol.*, 23, 1912, 218.

[7] *V.* also Wundt, W., *Grundriss d. Psychologie*, Leipzig, 1901, 28, and Ebbinghaus, H., *Grundzüge d. Psychologie*, Leipzig, 1903, 578, for similar uses of *Hilfsmittel*.

[8] Wundt, W., 'Selbstbeobachtung und innere Wahrnehmung,' *Philos. Stud.*, 4, 1888, 296.

With this we can close our description of those factors which in general characterize experimental psychology and, according to our examination, distinguish it. We have yet to consider the *method* [*Methode*] to whose application experimental psychology owes its name. Experimentation is not a new source of psychological knowledge for psychologists, but, as has been emphasized often enough on all sides, it is an important aid [Hilfsmittel] in the exhaustive exploitation of sources which are already available.[1]

There is evidence, then, on the one hand, that under *method* we may expect to find a variety of meanings, and, on the other, that in the usage of several different words we are able to trace a common, though loosely defined, reference. In the one case, *method* carries several distinct interpretations; in the other case, various words are used as equivalents for a single meaning. The empirical study of a large number of systematic treatises in psychology reveals, first of all, at least four more or less distinct interpretations in the use of *method;* but it also shows that a trace of these different interpretations is likewise to be found in the several words which are used as synonyms for *method.*

The most frequent meaning of the term is: (1) a general mode of investigation. Examples of this interpretation are: *introspective method, method of observation, behavior method, experimental method.* This group includes all of those more important and typical methods which orientate the science in its investigation and which characterize the general attitude taken in the envisagement of phenomena. These methods are typical, furthermore, of the science considered as a whole, *i. e.*, pertaining to the entire field of psychology. If, any one of these methods, once admitted, were to be discarded, a total change in the point of view, or at least a very radical limitation in the kind of phenomena which are considered psychological, would result.

The name by which the self-ward looking of the psychologist is designated is introspection. In so far as it tends sharply to contrast physical with psychological methods of observation this special term is unfortunate, for as a matter of fact there are no fundamental differences in the methods of the two sciences.[2]

They are methods [experimentation, introspection, etc.], in short, of elevating us above what is purely contingent and accidental in self-consciousness, and revealing to

[1] Külpe, O., 'Anfänge und Aussichten der experimentellen Psychologie,' *Archiv f. Philos.*, 6, 1893, 454.

[2] Yerkes, R. M., 'Introduction to Psychology,' New York, 1911, 41.

us what in it is permanent and essential; what, therefore, is the subject-matter of psychology.[1]

A meaning of *method* which is subsumed under the first rubric and which is, in a sense, a subclass under it, is: (2) a specific type or order of procedure for purposes of control or treatment. Instances of this class are: *method of impression*, *method of average error*, *time-limit method*. The term as here used usually denotes a refinement of experimental method, or of some other method of the first class, and means the manner of handling the material in the control of the investigation. Its application is circumscribed by a particular subject under observation, or by a limited range of attack. It is not applicable to the whole science as such, does not modify the main point of view of the discipline, and can, therefore, be omitted from the list of methods without serious handicap to the science. This type of method, in conjunction with a large number of similar methods, nevertheless, contributes data to psychology.

In the case of the method of minimal change the tests above and below the standard, both working toward it and away from it, must be so alternated as to bring each stage of the test into as nearly like conditions of attention and fatigue as possible; at least when accuracy is an object.[2]

Now, in theory, rate of speed might be measured either by the amount of work performed within a given time or by the time taken to perform a given amount of work, in other words, by a *time-limit* method or by a *work-limit* method.[3]

The word *method* is used in still another sense: (3) the point of view taken, or the intention assumed, in an investigation. This is shown in such designations as: *genetic method*, *comparative method*, *indirect method*. Methods of this class indicate the purpose which prompts the investigation, the attitude assumed in the scientific study. *Genetic methods* would therefore indicate an attitude toward conscious phenomena which has to do with their development and history; these methods serve to reveal facts concerning the genesis of mind. So comparative methods involve a way of viewing mind in the light of its development and of its manifestation in other than

[1] Dewey, J., 'Psychology,' 3d ed., New York, 1891, 13.
[2] Sanford, E. C., 'Course in Experimental Psychology,' Boston, 1908, 348.
[3] Whipple, G. M., 'Manual of Mental and Physical Tests,' Baltimore, 1910, 6.

human organisms. Indirect methods carry the premise that the attack is not inherent in the discipline under whose auspices the method is carried out. In some respects this class of methods resembles the first group; but there are marked differences. The first class has its warrant in the kind of approach; the third class specifies the purpose or attitude behind the approach. The experimental method, for example, when used for the purpose of describing the development of mind in the child, becomes genetic in character; when it is used as a means for timing the reflex arc it becomes indirect; when it is used for the purpose of timing the reflex arc in the child and in the adult for the purpose of deducing genetic relationships, it is both indirect and genetic. Naturally, in a way, experimentation, introspection, and the observation of behavior involve a definite point of view at large, but the words used as adjectival modifiers do not specify the purpose or attitude; nominally the method does not bear the identification necessary to the classification.

This historical unfolding of the course of psychical development is most properly described as the genetic method, that is to say, the method which exhibits the genesis or becoming of things.[1]

The *comparative method*, finally, supplements the introspective and experimental methods. This method presupposes a normal psychology of introspection to be established in its main features. But where the origin of these features, or their dependence upon one another, is in question, it is of utmost importance to trace the phenomenon considered through all its possible variations of type and combination.[2]

The last type of method is sharply demarcated from the others in that, while the latter were classified in terms of empirical differences in the scientific operations involved, it is based on the logical interpretation of these operations. *Method* in this sense is used as: (4) the type of reasoning involved in any of the three preceding forms of operation, or in the systematization of the results obtained, as in: *inductive method*, *method of deduction*, *synthetic method*. As in several other classes, methods here enumerated are not peculiar to psychology, but are characteristic of other sciences as well. Logical analysis of the forms of thought-processes employed

[1] Sully, J., 'The Human Mind,' Vol. 1, New York, 1892, 28.

[2] James, W., 'Principles of Psychology,' Vol. 1, New York, 1890, 194.

in the application of any or all of the other three types of method furnishes the distinguishing principle upon which the fourth class is founded. Such methods,—of generalization, of classification, and of systematization,—are also conspicuously used in the erection of systems of psychology and of the general laws which follow from the operation of other methods.

> To observation, direct and indirect, and to analysis by introspection, reflection, and experiment, we add *induction*—as the necessary method of psychological science. . . . Here, of course, the so-called inductive method implies—strictly speaking—deduction as well as induction, and both analysis and synthesis, after the fashion of the science-making mind of man.[1]
>
> Of the two great historic methods, deduction and induction, recent investigation has shown that neither alone is exclusively productive of great results in the way of discovery or construction, though in their general characteristics and predominant importance, induction may be said to be the method of discovery and deduction the method of construction.[2]

We are now prepared to undertake a comparison of all of these four classes of method in terms of a schema. It is clear that the term *method* has at least four usages. On the principle that it is always bad practice in the field of science to use an expression multivocally, the following proposals are made in regard to the adoption of substitutive phrases for the several meanings of *method:* (1) that *method* be reserved for the first class, *e. g.*, method of experimentation; (2) that *procedure* be used for the second class, *e. g.*, procedure of average error; (3) that *point of view* be substituted for the third class, *e. g.*, comparative point of view; and that *rational principle* be substituted for the fourth class, *e. g.*, rational principle of generalization.[3]

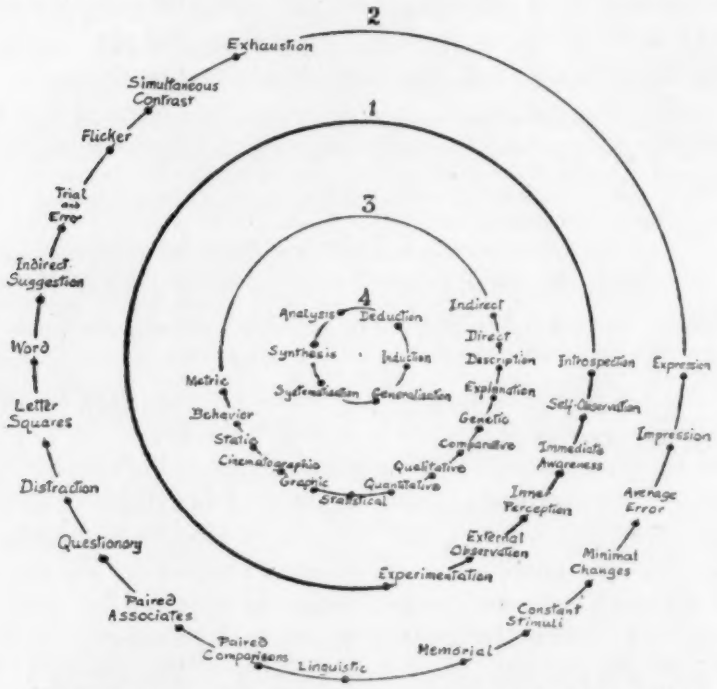
The entire schema then becomes a hierarchy which may be best represented by a fourfold system of orbits, on the pat-

[1] Ladd, G. T., 'Psychology, Descriptive and Explanatory,' New York, 1895, 24–5.

[2] Baldwin, J. M., 'Handbook of Psychology,' Vol. 1, 2d ed., New York, 1890, 20.

[3] Any proposal to modify a practice upon which tradition heavily rests will naturally meet with some resistance through the inertia of the usage: language is not usually so radically reformed or revised as spelling seems to be. But it is hoped that a statement of the question at issue will raise a sane discussion of the merits of the case. In this event, though agreement be not reached, a step will have been taken toward awakening in the writers of text-books and systematic treatises a realization of the necessity for accurate expression in matters of terminology. At any rate, a more pronounced self-criticism in this regard is not too ambitious a standard to erect.

tern of circular slide-rules, but with the added possibility of changing the relative positions of terms in the same orbit. Such a schema is exhibited in the diagram:



Numbers indicate the order in which these classes are discussed. Orbit 1 is heavily lined to show that it represents the class of true *methods*.

On the inner orbit the logical 'methods' or *rational principles* are located, radiating their influence away from the center through all of the other methods. Then come the *points of view*, in turn affecting the operations which lie beyond, but capable of altering their positions so that one *point of view* may be brought in radial position with any *method* or *procedure*. So, likewise, the *methods* may shift positions in the third orbit, bringing any one of the *procedures* in the fourth orbit into radial proximity. As the diagram stands, for instance, the procedure of expression may be used under the introspective method, with a descriptive point of view, and according to the rational principle of induction. The classi-

fication is necessarily incomplete because with the inclusion of other psychological treatises new operations will have to be added. By far the greatest number of operations occur in the orbit of procedures. From within to without a greater refinement is to be noted from the general to the particular.

In conclusion and summary, then, an empirical survey of the literature reveals four classes of usage of the term *method:* (1) as a general mode of investigation, (2) as a specific type or order of procedure for purposes of control or treatment, (3) as a point of view taken, or the intention assumed in an investigation, and (4) as the form of reasoning involved in the pursuit of any of the preceding types of operation, or in the systematization of the results obtained. For these four types of operation it is suggested that the terms, (1) *method*, (2) *procedure*, (3) *point of view*, and (4) *rational principle* be substituted. These classes may be best represented in an orbital diagram or schema in which they assume positions ranging from *rational principles* in the center through *points of view* and *methods*, to *procedures* on the outside.

FATIGUE IN A COMPLEX FUNCTION

BY EDWARD L. THORNDIKE

Teachers College, Columbia University

By the coöperation of eighty-nine students in a graduate course in educational psychology at Teachers College I am able to report measurements of the effect of about four hours of continuous work at writing poetry upon the quantity and quality of the product produced per unit of time and upon the satisfyingness of the process of producing it.

The work consisted of writing lines to complete 108 couplets, the first lines being given. These first lines were taken from Pope and Byron, the following being a random sampling.

Glittering with ice here hoary hills are seen
_____

The fourth day rolled along and with the night
_____

Self-love forsook the path it first pursued
_____

What she has done no tears can wash away,
_____

Bid harbors open, public ways extend
_____

From the damp earth impervious vapors rise
_____

Mark first that youth who takes the foremost place
_____

Back to my native moderation slide
_____

But still he only saw, and did not share,
_____

In clouded majesty here dullness shone;
_____

But while he shuns the grosser joys of sense,
_____

But high above, more solid learning shone
_____

They were arranged in 9 sets of 12 lines each (called hereafter sets *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h* and *i*). Eight sets were done

without rest in the afternoon or evening of a given day; the ninth set being done after rest in the morning or afternoon of the following day. The individuals who engaged in the experiment were divided into nine squads (called hereafter squads *abc*, *bcd*, *cde*, *def*, *efg*, *fgh*, *ghi*, *hia* and *iab*). One squad did the sets in the order *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, rest, *i*; the next squad did the sets in the order *b*, *c*, *d*, *e*, *f*, *g*, *h*, *i*, rest, *a*; and so on. I shall use Period 1, Period 2, Period 3, etc., to designate the nine periods of work. The arrangement of the work was then as in Table I.

Each individual recorded the time required to get the twelve couplets of each set, and also the degree of satisfyingness of the work on each set. The scale for satisfyingness was arbitrarily defined as follows: Call 5 the amount of enjoyment or satisfyingness of mental work which represents your average condition; let 10 represent the greatest amount

TABLE I

THE SET OF COUPLETS COMPLETED BY EACH SQUAD IN EACH PERIOD

Squad	Period								
	1	2	3	4	5	6	7	8	9
abc	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*
bcd	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*a*
cde	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*a*	*b*
def	*d*	*e*	*f*	*g*	*h*	*i*	*a*	*b*	*c*
efg	*e*	*f*	*g*	*h*	*i*	*a*	*b*	*c*	*d*
fgh	*f*	*g*	*h*	*i*	*a*	*b*	*c*	*d*	*e*
ghi	*g*	*h*	*i*	*a*	*b*	*c*	*d*	*e*	*f*
hia	*h*	*i*	*a*	*b*	*c*	*d*	*e*	*f*	*g*
iab	*i*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*

of enjoyment or satisfyingness which you have experienced from mental work; let 0 represent the greatest distaste or intolerance toward any piece of work which you have experienced. Let 6, 7, 8, 9 and 1, 2, 3, 4 represent intermediate conditions by equal steps. This scale is obviously crude and unduly subjective, but will suffice for such inferences as will be drawn here.

The quality of the poetry written was measured as follows: The various completions of one couplet (call it *a* 1) were collated and graded by four judges. The same four judges

would grade similarly the completions of another couplet (call it *a* 2). A new combination of four judges would grade the completions of *a* 3 and *a* 4; a new combination would grade the completions of *a* 5 and *a* 6. In all about 80 judges shared in the work of grading the couplets for quality, each judge grading some hundred or more lines; and every line being graded by four judges. The grading was on a scale of 0 to 10, 0 being a line absolutely devoid of merit and 10 as good a line as, in the opinion of the judge, could be written to make that couplet. This again is obviously a very crude and unduly subjective scale, but will serve the purpose of the present argument sufficiently well.

We have then for each of eighty-nine individuals a record like the following of the time required for each of eight consecutively accomplished sets of 12 lines of poetry, and of one done the next day after rest, of the satisfyingness of the work at each of the nine periods, and of the quality of the product produced.

Individual M. D. F. did set *f* in period 1, 7.46.30 to 8.5.45 P.M., spending 25 minutes 15 seconds, the successive couplet-completions being rated as 16, 17, 21, 15, 14, 17, 17, 9, 6, 13, 13 in quality by the sum of four judges' ratings. M. D. F. rated the satisfyingness of the work as 4.

He did set *g* in period 2, 8.8.45 to 8.29.30 P.M., spending 20 minutes 55 seconds, the successive couplet-completions being rated as 14, 19, 20, 11, 14, 16, 20, 15, 12, 7, 18, 13 in quality by the sum of four judges' ratings. M. D. F. rated the satisfyingness of the work as 4.

And so on for the seven remaining sets.

These individual records are arranged in 9 squads so that each set (*a*, *b*, *c*, *d*, etc.) was done at each period, equalizing the effect of any differences in the difficulty of the sets of first lines to be made into couplets.

Examination of the individual records shows that for the problem under consideration there is no misleading in massing the results and presenting simply average or median achievements and degrees of satisfyingness by squads. This is done for time required, quality of product, and reported

satisfyingness in Tables II., III. and IV., which utilize all the records for Period 1, Period 8 (the last of the consecutive work-periods), and Period 9 (the work-period of the following day after rest).[1]

The facts are clear. The speed of work increases throughout the work-period and is not benefited by the rest. The average quality of the product produced falls off a very little, from 4.47 in Period 1 to 4.24 in Period 8, and is slightly benefited by the rest, from 4.24 in Period 8 back to 4.47 in Period 9. The reported satisfyingness of the work falls off greatly, from 5.38 in Period 1 to 3.56 in Period 8, and is greatly benefited (from 3.56 to 4.85) by the rest. The effect of continuous exercises of the function is to increase gross efficiency, but to decrease satisfyingness or interest. The effect of the rest is a very slight gain in gross efficiency but a very great gain in satisfyingness or interest.

TABLE II

TIMES REQUIRED TO COMPLETE SETS OF 12 COUPLETS AT THE BEGINNING OF THE WORK-PERIOD (PERIOD 1), AT THE END OF THE WORK-PERIOD (PERIOD 8), AND ON THE NEXT DAY AFTER REST (PERIOD 9)

Squad	Period 1	Period 8	Period 9
abc	29.4	19.3	21.7
bcd	37.4	20.7	21.9
cde	39.3	22.6	22.4
def	35.1	23.3	22.6
efg	44.5	27.0	29.7
fgh	36.4	24.6	23.7
ghi	27.7	22.8	25.3
hia	30.4	28.3	23.5
iab	30.0	18.3	17.8
Average	34.5	23.0	23.2

[1] Some of the eighty-nine individuals did not complete all of the 108 couplets within the approximate four hours set apart for the experiment. Some others by accident failed to complete the entire set of couplets or to score the time.

In all there were fourteen such individuals out of eighty-nine making 27 such blanks out of 801. Where an individual did not do all eight sets in the work period the last set that he did do was treated as the set to be counted under Period 8 in Tables II., III. and IV.

TABLE III

MEDIAN QUALITY OF POETRY WRITTEN AT THE BEGINNING OF THE WORK-PERIOD (PERIOD 1), AT THE END OF THE WORK-PERIOD (PERIOD 8), AND ON THE NEXT DAY AFTER REST (PERIOD 9). THE QUALITY MEASURE IS THE SUM OF RATINGS BY FOUR JUDGES

Squad	Period 1	Period 8	Period 9
abc	15.5	13.0	14.5
bcd	19.0	17.0	19.0
cde	18.5	17.0	18.0
def	17.0	18.5	20.0
efg	18.5	17.0	17.0
fgh	18.5	16.5	17.5
ghi	20.0	19.5	19.5
hia	17.5	17.0	18.0
iab	16.5	17.0	17.5
Average quality	4.47	4.24	4.47

TABLE IV

REPORTED AVERAGE SATISFYINGNESS OF WRITING POETRY AT THE BEGINNING OF THE WORK-PERIOD (PERIOD 1), AT THE END OF THE WORK-PERIOD (PERIOD 8), AND ON THE NEXT DAY AFTER REST (PERIOD 9)

Squad	Period 1	Period 8	Period 9
abc	4.75	3.42	4.42
bcd	5.55	4.89	5.67
cde	5.30	3.40	4.90
def	4.20	3.70	4.70
efg	5.09	2.64	3.91
fgh	5.75	3.52	4.86
ghi	6.50	3.88	5.38
hia	5.86	2.71	5.71
iab	5.43	3.86	4.14
Average	5.38	3.56	4.85

THE CURVE OF WORK

The average amount per unit of time and satisfyingness reported for each successive period of the total work period and the quality of the product in periods 1, 4, 6 and 8, were as follows:[1]

[1] The results given here are not for equal *time* periods but for successive sets of twelve of the *couplets*. Also there were necessary a few adjustments in the case of the 3½ per cent. of blank records referred to in a previous note. The average times of periods 1, 2, 3, 4, etc., were approximately 35, 28, 26, 25, 25, 24, 24 and 23 minutes.

Period	1	2	3	4	5	6	7	8
Couplets per 10 minutes	3.48	4.36	4.69	4.82	4.84	5.08	5.03	5.22
Quality of couplets	4.47	...	...	4.43	...	4.35	...	4.24
Satisfyingness	5.38	5.21	4.94	4.48	4.31	3.95	3.67	3.56

The changes from period to period were thus:

	1–2	2–3	3–4	4–5	5–6	6–7	7–8
Couplets per 10 minutes	+.88	+.33	+.13	+ .2	+.24	−.05	+.19
Satisfyingness	−.19	−.27	−.46	−.17	−.36	−.28	−.11
Quality of couplets	−.04 from 1 to 4			−.08 from 4 to 6		−.11 from 6 to 8	

These facts are also clear. Speed improves, with fluctuations such as one customarily finds. Quality remains about the same. Satisfyingness falls off throughout.

ON THE READING AND WRITING OF MIRROR-SCRIPT

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§ 1. Report on a Case of Spontaneous Right-Hand Mirror-Writing

During the college year 1912–1913 the writer had under observation a third-year college student, twenty-five years of age, referred to as *X*, who reported some peculiar conditions existing relatively to a long-continued habit of *right-hand* mirror-writing. Study of the case suggested certain significant questions of general interest in connection with the problems of visual and motor orientation. The purpose of this paper is to state these problems and to discuss them briefly in the light of the data at hand.

The facts reported by *X* were obtained from answers in writing to a set of questions submitted by the experimenter. A duplicate set of questions was sent to *X*'s mother in another town and the independently obtained answers compared.

X, it appeared, learned to write at about five years of age by copying the writing or printing of her mother who sat across the table from her. After watching the mother's movements across the table, *X* would be given the copy to imitate. *X* wrote mirror-fashion in these early attempts and during the whole of her first year in school, during most of the second year, and a little during her third year. The teacher who succeeded in breaking *X* of the habit exercised great care, requiring written work to be done over and over until it resembled normal productions. Not until about ten years of age was the child thoroughly broken. *X* adds that even at the present time, when very tired, she reverts to mirror-writing.

X reported that she had a strong tendency to read from

right to left instead of from left to right and that this habit persisted about as long as the mirror-writing habit, adding, "I don't believe I am really broken of it as I will sometimes read three or four lines that way before I think what I am doing." There was a tendency at first to pronounce words backwards, "then as I grew older and could recognize words such reading affected merely the sequence of words." During the time she was under observation, *X* complained of difficulty in mastering first-year French, due in part to attempts to pronounce the words according to the sequence of letters from right to left. *X*'s mother reported in addition to these facts that in *X*'s first attempts at number-work the answers were placed on the top of the examples. Also, that *X* had, as an infant, crawled backward.

X's further observations were very significant. "I worked in my cousin's newspaper office for about a year. It was no trouble at all for me to learn to set type. Within a month I could set type as fast as my cousin could, although he had worked at this ten or more years."

"When handwriting is not familiar to me I have to spell most of the words out. After a month or so of teaching I could read the writing of my pupils readily but was never able to tell which pages belonged to which child when no name was signed. Such a condition held even after two years' work with certain children. I would always have to spell out names that were not written very plainly—every letter distinctly." *X* reports, further, that she cannot recognize differences in the writing of her father, mother, and an older sister.

X was unable to pass an examination for post-office work which consisted in reading a given number of addresses in a given time. She served, however, as a postmistress for a year before teaching and was obliged to give up this work because of the great nervousness ensuing, due to the strain involved in the reading of many different hands.

The contrast in the ease with which *X* took up type-setting and the difficulty she found in post-office work and in general in the reading of script is instructive since it points

to the greater facility with which she makes shifts in position in comparison with shifts in form.

Again, *X* reported that in memorizing a page it is placed preferably to the right; the page is then memorized in visual form but turned over and in front, that is, in the imaginal representation, the page is seen in mirror-fashion. To copy this, the page is again turned over in imagination but now towards the left. In reciting orally, *X* reads from the front page where the type is seen in reversed form. In order to memorize a selection to the best advantage, *X* copies it on a blank sheet of paper using one side only as there is a conflict if both sides are written upon. If both sides are written upon the first sheet is more easily visualized than the second.

The points of interest in this general report are (1) the association of spontaneous right-hand mirror-script, induced perhaps by the method of learning to write, with general confusion in the matter of motor orientation; (2) the ease with which X_1 according to her report, is able to handle shifts in position of letters in comparison with shifts in their form; (3) the suggestion that the mirror-reversal may also affect imaginal representations.

Before formulating questions relative to the general significance of such reported facts the writer made an attempt to test these assertions of *X* and, also, to determine by means of control tests on other reagents the extent to which *X* showed unusual capacity in dealing with displacements with subcapacity in dealing with differences in form.

In class work with *X* the writer noticed some peculiar conditions, for instance, a quick verbal memory with considerable incapacity in dealing with meanings, a point of some interest as will be seen in a later connection. Shifts in terminology always confused *X* greatly. Furthermore, although timed tests upon rapidity of writing showed that *X*, in comparison with other students, is a rapid and excellent penman, she composes with very great slowness, in fact so slowly as to indicate some inhibiting tendency at work. It will be recalled that *X* reported that when fatigued she has a tendency to revert to mirror-writing. She was also of the

opinion that she would be better able to compose if allowed to write in mirror-script.

The following test was tried. *X* was given two questions in a class quiz. The first question she was instructed to answer in normal script, the second, in mirror-script. These answers were completed in twenty minutes. Then, unanticipated by *X*, the instructions were reversed: the first question was to be answered in mirror-script; the second in normal script. Again, twenty minutes were consumed. The results did not seem to confirm *X*'s opinion. In both cases the questions were answered in mirror-script less fluently than in the normal. 140 words were written in normal script in answer to the first question; 120 words in mirror-script. In the case of the second question, 52 words were written in mirror-script, 115 words in the normal. There was no evidence of greater ease of thought in writing the mirror-script. The second twenty minutes produced 235 words as over against 192 words in first twenty, but with considerable repetition of thought and reversion to the earlier wording. The test served, however, to show definitely the slowness with which composition proceeds and points to a retardation of some kind.

Memory tests on *X* were not carried to completion. Many times, however, in the course of these as well as in tests on writing under distraction *X* reported a visual verbal image which was seen in reversed form. In general, it may be noted that *X* makes much use of visual imagery. Specifically, in the memory tests, this reversed image appeared largely when *X* sought to memorize a passage by first *writing* it out and then memorizing it visually.

A test on the memorizing of two passages in prose, which *X* first wrote on a sheet of paper, gave some interesting results, which, however, need confirmation by further work. Briefly, the following points were noticed. The first passage, one of eight lines, was written by *X* upon a blank sheet of paper. When *X* thought that she knew the passage she wrote it again and compared her reproduction with the original passage, concentrating upon the more difficult parts. When confident of the whole she repeated it to the experi-

menter, reading from a visualized copy of the passage as it appeared the time of her second writing, the whole being seen on a level with her eyes, from the other side of the paper, that is, in mirror-script. Ten and a half minutes were taken for memorizing the passage; four trifling errors were made in the reproduction.

The second passage, of eight lines taken from the same book as the first, was memorized as before except that it was written upon a sheet which contained writing on the reverse side. The memorizing of this passage required eleven and a half minutes, with eight errors in reproduction, much more serious errors than occurred before. The content may, of course, have been responsible for the increased difficulty. The passage was read off in mirror-script as in the preceding test but certain words were blurred. *X* reported that, so far as she was able to judge, the writing on the other side of the paper had nothing to do with this blurring. The memory was at fault. The experimenter noticed that *X* turned greatly to the left in attempting to recall the passage. This turning to the left appeared also in a second reproduction of the first passage. When instructed to turn to the right and repeat the second passage, *X* hesitated for fifty seconds and then said, "Picture won't come!" Directed to turn toward the left, after thirty seconds' hesitation, she was able to see certain words and to give correctly the last line as a whole. Turning towards the right caused a blurring of this line. A third repetition of the first passage when *X* was turned first towards the right, then towards the left showed greater control of the visual imagery in the second case with the giving of words that had been blurred during the preceding visualization.

These observations are included in the report with some hesitation since the writer is conscious of their inadequacy. They suggest, however, interesting lines of investigation.

X's capacity in the reading and writing of mirror-script was determined by tests in which her skill was compared with that of eight other third-year college students. Later, as will be described in another section of the paper, these

tests were made much more extensive but with little additional information so far as *X*'s comparative skill was concerned.

The first test tried was on the rapidity with which a reagent could read a sentence written in large formal mirror-script. This test will be described in detail further on in the paper. At this point in the discussion the following observation alone is important, namely, that among the nine reagents compared, there are four who read the sentence more quickly than *X*, four who read it more slowly. The exact records of this group are given in Table I., Group II. The range in time is, it should be noticed, very great, extending from 33 seconds to over four minutes. *X*'s time is two minutes and fifteen seconds.

A second much more difficult test on the reading of mirror-script was tried on seven of these reagents, *X* being one of the seven. In this test a postal-card was utilized containing some fifty words written in mirror-script that possessed the striking individuality of usual writing. The time needed to read this card ran from twenty-seven minutes to over an hour. Among these seven reagents *X* takes third place, the two who excel her, and that slightly, being those who occupied the first and third place in the preceding test. On the whole, *X* shows greater comparative efficiency in this test than in the first.

In the third test mirror-type was utilized. Again, *X* was third of six subjects. Throughout these three tests there is very little shifting in relative position. Subject *B*, who showed great proficiency in the ease with which he read mirror-reversals, maintained first place in all these tests. *X* was less able to utilize meaning in her reading than were certain other of the reagents. She also complained of the peculiarity of the writing on the postal-card, and repeated her assertion that she always found difficulty in reading a strange handwriting.

On the whole, it appeared from the tests that while *X* showed a certain amount of efficiency in the reading of mirror-script, there were unpracticed reagents who excelled

her. The test served, however, to emphasize the extraordinary differences found among individuals in their ability to deal with the material under consideration.

In order to test *X*'s assertion that she found great difficulty in reading unfamiliar writing or writing that in any way varied from formal script, records were made of the rapidity with which she and the eight other reagents of the first mirror-reading test were able to read a letter written in a peculiar and somewhat difficult hand. This test was also utilized later in a more extensive way and will be described in some detail in an additional report. There were two parts to this test. In the first part the reading of the first page of the letter, consisting of seventy-one words, was timed. In the second part, the reading of the second page of the letter, seventy-one words as before, was timed. In this second test, however, an attempt was made to get rid of context and, hence, of meaning, by blotting out intermediate and significant words. Meaning was, however, by no means completely obliterated. A comparison of the results from the first and the second test for any particular reagent served to bring to light the relative dependence upon meaning in the first and upon visual habituation in the second case. These individual differences will be discussed in another connection. So much may be said here. The range in time for reading the first page ran from 57 seconds to six minutes. *X*'s time was four minutes fifty seconds, with five errors. Four of the reagents were more rapid; four were slower. In the second test, seven were more rapid than *X* and only one was slower. To a slight extent, then, *X*'s opinion was confirmed. Furthermore, when tested on her ability to identify specimens of handwriting produced by the same penman, *X* proved to be the most inaccurate of the group with one exception.

These tests served to throw into the foreground extraordinary individual differences and suggested a series of tests which the writer has at present under way. The comparative rating of *X* in these tests and in those on mirror-reading cannot, of course, be cited in any conclusive way. The situation suggests, however, a problem of some interest,

namely, the ability to deal with position as distinct from the ability to deal with form.

X's skill in mirror-writing was also determined by control tests upon eight other reagents. These nine subjects all wrote the same familiar verse, first, with the right hand in normal script; second, with the right hand, mirror-script. Their rapidity was timed with a stop-watch. The test was then repeated with the left hand.

Of the nine subjects, there were two who wrote the given verse slightly more rapidly (25 and 14 seconds) than *X* in the right-hand mirror-script; there was one who was more rapid in the left-hand mirror-script. These subjects were not those who had excelled in the reading of mirror-script. In both cases, however, *X* produced copy that was superior to that of the more rapid reagents. *X*'s mirror-script, however, although of fair quality, does not compare in appearance with her normal right-hand writing. Moreover, she produces it much more slowly than the normal as shown by the fact that the given verse was written in the usual manner in 59 seconds, in mirror-script in three minutes eleven seconds. *X*'s normal writing with the left hand showed no superiority either in rapidity or appearance; the mirror-writing, left hand, four minutes three seconds, compared favorably with that of the other reagents.

As a control in both left- and right-hand mirror-writing, *X* reported that she visualized the verse *as a whole* in mirror-script and copied this verse word by word without attempting vocalization of the words. Visualization in mirror-script of the more difficult words appeared also when *X* attempted to write the verse in normal fashion but under various forms of distraction. *X* was able to maintain mirror-writing both while counting aloud and while reading aloud with no more effort than was found necessary in maintaining normal writing under such conditions.

As a net result from the tests we may conclude that, in spite of her habituation to mirror-forms, *X* shows no striking superiority to certain unpracticed reagents. Such a conclusion raises a number of general questions: (1) the extent

to which individuals differ in their capacity to interpret mirror-reversals; (2) explanation of such individual variation; (3) the extent to which ability in reading mirror-script (visual) is correlated with ability to write it (motor) and with ability to read and to write inverted script; (4) the relative skill of the right and the left hand in the production of mirror-script; (5) the relation of capacity in mirror-reading with capacity to interpret form in general. Before citing tests that attempt to answer the first four questions, it may be well to review certain experimental studies that throw light upon the general problem.

But, first, a word concerning the appearance of mirror-reversals in imaginal form.[1] Janet in an interesting article has suggested that reversal of position in one's visualization of a situation may account for certain illusions of orientation. The writer of this paper has had occasion to note in two or three imagery tests left-right inversions of objects in a visualized whole. The point is emphasized here as one that merits further investigation.

§ 2. Literary Setting of Problem

An increasing interest in the tendency of certain children to write mirror-script instead of normal left-right script is shown by the reports on the subject that have been multiplying within the last ten years. The chief value of these reports has consisted in determining the conditions under which spontaneous mirror-writing appears and in proving that the early conclusion that mirror-writing is the normal writing of the left-handed or of the left hand in general when the right hand becomes incapacitated is an inadequate generalization.

Stern[2] has furnished an illuminating discussion of the subject and succeeded in bringing it in line with the general problems of space perception and motor orientation.

[1] Janet, P., 'Le renversement de l'orientation ou l'allochirie des representations,' *Jour. de psychol., norm. et path.*, 1908, V., 89–97.

[2] Stern, W., 'Ueber verlagerte Raumformen,' *Ztscht. f. ang. Psychol.*, 1909, II., 498–526.

Stern has shown, by his own observations and those of others, that spatial displacements are common occurrences in the early drawings and attempts at writing of the right-handed child as well as of the left-handed one. Furthermore, a point which needs emphasis, the right-left displacement resulting in so-called mirror-writing or mirror-drawing is not the only form of shift in position that occurs. A child in first attempting to make numerals may, for example, turn them completely upside-down or may turn them toward the right 90 degrees so that they appear lying upon the side. Nor is the problem one that concerns motor phenomena exclusively since the child also shows ability in interpreting perceptually forms that are shifted from their normal position. Various observers from Sully on have reported that the young child finds no difficulty in enjoying pictures that are held upsidedown or otherwise turned from their usual position.[1] This apparent indifference of the young child to position can, Stern insists, be understood only in the light of the development of space perception. There exists in the child a perception of form with an apparent indifference to position that seems to indicate that while form is nativistically determined, perception of position is an outgrowth of experience. Aboveness, belowness and the other directions represent not pure optical data but originate through the association of determined optical impressions with certain movements of the body. In the child fusion of form and position has not yet resulted, hence the ease with which he produces and interprets given forms in any position. Even in the adult this connection does not have the stability of inborn sensory connections.

The further question why variations occur in the propensity of children to produce such forms is raised. Partly, Stern insists, these variations depend upon external conditions which may make permanent a form of displacement originating in some accident. Partly, these variations are thought to be dependent upon inner conditions, since the child visually preoccupied will pay less attention to the posi-

[1] A child of two years whom I tested recently recognized without difficulty in an inverted picture faces measuring only one eighth of an inch across.

tion of a given form than will the child of a more motor type who comprehends optical representations egocentrically, that is, with reference to movements executed in their presence whereby their position becomes fixed. Indifference toward spatial position may also be due to failure to assume a practical attitude toward a seen object,—an interesting point in connection with *X*'s failure to reckon with meanings. The more space-forms take on a practical social aspect, the more their position becomes an integral part in the comprehension of the whole. While production of mirror-writing is in itself no sign of deficiency, a continued failure to note its difference from usual writing may indicate weakness of comprehension. Mirror-writing with the left-hand may also result, Stern holds, as a consequence of practice in normal writing with the right hand.

Stern's formulation of the shifting relation between spatial form and spatial position in the course of development is very suggestive. Individual variations even in children relative to the closeness of the association of these two elements arouses the suspicion that in adults also variation may exist as to the degree of stability of the connection, a conclusion in line with the observations reported above.

In this connection the experiments of Paula Meyer[1] upon the memory for simple forms are in point. Meyer gives the percentage of cases in which figures reproduced from memory are reproduced in some form of reversal and finds, in support of Stern, that such reversals of form occur more frequently in the case of children than of adults if reversals of all kinds are considered. So far as the mirror-reversal is concerned specifically, the tables show certain conditions under which mirror-reversal has occurred more frequently in the case of the adults than in the case of the children. Since, however, reagents as well as conditions were shifted in the various series of tests and the reagents in each particular series were few in number, it is quite possible to conjecture that individual variation influenced the outcome to a considerable degree, a point, of course, worth clearing up.

[1] Meyer, P., 'Ueber die Reproduktion eingeprägter Figuren u. ihrer räumlichen Stellungen bei Kindern u. Erwachsenen,' *Ztschr. f. Psychol.*, 1913, 64, 34–91.

In any case, Meyer finds the mirror-reversal a more frequent occurrence than the up-down displacement. Dearborn[1] in testing recognition under objective reversal had found, on the contrary, that "an object is recognized more readily when inverted than in either of the two intermediate positions of quarter-reversal, and more readily than in the erect mirror-position or that position inverted." He found the next most favorable position for recognition to be that of the erect mirror-position. It is quite possible, of course, that the form of displacement most easily recognized may not be that which most frequently occurs in a motor reproduction. The discrepancy, however, deserves attention.

From neither of the above reports is it possible to determine to what extent individual variations occurred since the results are presented in massed form. As suggested above, Meyer's separate tables indicate, it would seem, considerable individual difference in the tendency to produce mirror-forms in the case of both adults and children.

Although both reports give certain individual introspections, it is, again, not possible to determine whether there exists any correlation between imaginal type and frequency of reversal in reproduction or ability to recognize a figure when so reversed.

A general survey of the situation suggests, therefore, the same problems as those raised in the preceding section, particularly with reference to the extent of individual variation in the capacity to interpret and produce spatial displacements.

One recalls at this point another series of experiments, in which so-called mirror-drawing has been utilized in order to test speed in the formation of new associations, or, more specifically, to test adaptability and its correlation with general intelligence (Burt),[2] the process of learning by trial and error and cross-education (Starch),[3] individual and sex

[1] Dearborn, G. V. N., 'Recognition under Objective Reversal,' PSYCHOL. REV., 1899, VI., 395–406.

[2] Burt, C., 'Experimental Tests of General Intelligence,' *Brit. Jour. Psy.*, 1909, 3, 94–177.

[3] Starch, D., 'A Demonstration of the Trial and Error Method of Learning,' PSY. BULL., 1910, 7, 20–23.

differences and practice effects (Whipple).[1] Calfee[2] has also utilized mirror-tracing in a recent test upon college freshmen with the suggestion that the discrepancy between her results and those of Burt may be due to a growth factor, a point of interest in the present connection. While these tests bear some relation to those just cited, there are some striking differences in the general situation, for in drawing or writing in which one perceives the result of the movement in the mirror, the readjustment required is that of an old movement to a new visual report, and success may be achieved by a voluntary ignoring of the visual report, while in production of mirror script the direction of movement is actually reversed.

The mirror-tracing tests also show great individual differences in the ease of adjustment. Whipple gives a time-range for adults in the first attempt at tracing the star from about 50 seconds to over 8 minutes. A sex difference is also suggested, indicating faster speed for girls and women than for men and boys.

The tests to be discussed in the following section of this paper make, of course, no claim to be an adequate treatment of the problems under consideration. They are concerned, chiefly, with certain features of interest in the reading and writing of mirror-script, with an extension to the reading and writing of inverted-script. Furthermore, an attempt is made to answer a few questions by working out certain correlation coefficients according to Spearman's foot-rule method as described by him in the *British Journal of Psychology*.[3] It is obvious, however, from current discussions upon the subject of correlation, that assertions based upon the use of any correlation formula must be more or less open to suspicion until a more general agreement is reached as to the demands of the method. No attempt was made to correct these correlation coefficients by a second test upon the same or

[1] Whipple, G. M., 'Manual of Mental and Physical Tests,' 1910, 343–349.

[2] Calfee, M., 'College Freshmen and Four General Intelligence Tests,' *Jour. Ed. Psy.*, 1913, IV., 223–231.

[3] Spearman, C., '"Footrule" for Measuring Correlation,' *Brit. Jour. Psy.*, 1906, 2, 89–109.

other reagents. A second test on the same reagents would have introduced habituation, which from my experience with reagents I judge to be very rapid in the case of mirror-writing but rather slow in the matter of reading of mirror-script.

It is, however, very possible that the determination of individual capacity in the matter of rapid habituation to either the reading or writing of mirror-script might prove of more value in the rating of individual capacity to deal with shifts of position than a ranking based on a first test. Furthermore, any striking difference in ease of adjustment to the motor test in contrast to the visual one would be very significant. Lack of time has prevented my carrying out such tests on habituation, although the report is incomplete without them.

§ 3. Individual Variation in the Ability to Read Mirror-Script

The reading of mirror-script was utilized in my monograph on control processes in modified handwriting[1] in the hope of throwing into the foreground certain individual methods of procedure. 3 *B*, Part I. of this monograph gives the reaction of nine subjects. Even among these reagents the individual variation in the ability to read a sentence in mirror-script was enormous, ranging from 40 seconds to over 25 minutes.

In order to obtain enough records to make certain comparisons possible seventy more records were obtained from as many different reagents, ranging in age from eight to seventy years. The manner in which these reagents were selected will appear later. Each reagent was tested separately and according to standardized instructions which were typed and placed before the experimenter. Five experimenters besides myself were utilized in giving the tests. All were members of my class in experimental psychology and worked under my personal supervision.

Method.—In order that results might be comparable with

[1] Downey, J. E., 'Control Processes in Modified Handwriting,' Psy. Rev. Mon., 1908, 9, No. 37.

those of the earlier test, duplicates of the card used in the monograph test were used in the present series. The sentence, "Two telephones were placed at two symmetrical points of the same circle," was written in large formal vertical script upon transparent white paper of good quality. This paper was then reversed and pasted upon a white card so that the script appeared in mirror-script. Before the exposure of the card, the reagents (except the children) were told that the time taken by them for the reading of a sentence written in mirror-script was to be obtained. If the reagent volunteered any questions concerning mirror-script, these questions were answered. As the card was turned into reading position by the subject, a stop-watch was clicked by the experimenter. Children were told that they were to read a sentence that ran from left to right (the direction being indicated by a gesture) instead of running in the usual direction of writing. They were told to spell out words if they chose. In every case an attempt was made to hold a reagent to the reading until every word had been correctly given. In a very few cases this proved to be an impossible task. On the basis, therefore, of the timed readings that had been obtained for individual words from a large number of reagents, the relative difficulty of the words was determined and arbitrary values assigned to each word. The rapidity for reading the whole was then calculated on the basis of the number of words read in the given time. These calculated values were, however, utilized in only five instances, and occur only in the case of excessively slow readers.

Results.—These seventy reagents show, as did those of the monograph test, surprising variation in ability. The range in time is from 25 seconds to about 47 minutes. This last record represents, it may be said, a calculated value since the reagent in question succeeded in interpreting only the simple words in the sentence.

There are 12 reagents, among the 79, who read the sentence in less than 60 seconds, with an average for this group of 38.9 seconds. There are 13 records that run over 8 minutes 30 seconds with an average of about seventeen

minutes. Very crudely, we may say, then, that the best group is about 26 times more able than the worst group.

The median value of these 79 measures is 3 minutes, 35 seconds and the approximate P.E., found as suggested by Whipple[1] "by counting off one fourth of the cases from either end of the series of measurements and halving the difference between the two values so found" is 2 minutes 2.5 seconds, that is, 39 cases fall within the values of 1 minute 30 seconds and 5 minutes 37 seconds.

Before considering attempts to relate such individual differences to other factors in the reagent's make-up, it may be well to discuss the possibility of practice effects entering into the results to any considerable extent. Every reagent was questioned as to whether he had, previous to the test, attempted to read mirror-script. Subject *X*, whose case was reported above, was the only subject who reported extensive practice. She ranks twenty-fifth among the seventy-nine reagents. A few others reported slight practice in the use of rubber stamps or in type-setting and one or two remarked that they had attempted to read, successfully or unsuccessfully as it might be, souvenir postal cards that contained sentences in mirror-script. The reagents who reported such practice made the following ranks: 1, 2, 3, 6, 25, 46, 56, 62. On the whole, an impression was obtained that a tendency to utilize or to amuse oneself with mirror-forms is to some extent indicative of readiness in dealing with them. As was suggested above, tests to determine the limits of practice for a large number of subjects are highly desirable.

In considering individual differences, the following factors seem to merit discussion, first, the influence of age, which as Stern's observations suggest, may determine the extent of fusion of visual and motor spatial factors; secondly, sex, since Burt and Whipple in the mirror-tracing tests find greater speed on the part of women and girls, a superiority which Whipple relates to the greater use of the mirror by women but which Burt is inclined to attribute in part at least to an innate sex-difference; thirdly, the influence of certain perceptual and imaginal predispositions.

[1] Whipple, G. M., 'Manual of Mental and Physical Tests,' p. 18.

I am inclined to believe, further, that family resemblances deserve study in this connection. Striking similarities in reaction would point to innate difference as basal in the explanation of readiness or awkwardness in dealing with spatial reversals. The following facts may be of interest. Four members of the writer's immediate family took part in the above test. One ranked thirty-ninth, the others, sixty-third, seventy-third, and seventy-sixth. Two other members tried after the completion of the test, after staring helplessly for three minutes at the test-card without succeeding in reading even the simplest words, refused to make further effort. One remarked that he had often tried to read mirror-typewriting but always unsuccessfully. Two of the slowest of the subjects, occupying the seventy-fifth and seventy-seventh places, are sisters. The reagents ranking respectively sixty-six and seventy-four are mother and son; those ranking second and twenty-second, brother and sister; those ranking twenty-fifth and sixty-fifth sisters.

Should striking innate individual differences, beyond the influence of practice effects, be found, they might well prove of practical significance as well as of theoretical interest in their bearing upon the problems of spatial orientation. It may be conjectured that the difficulties of certain students in dealing with stenographic symbols may be explained by further investigations along this line. Varying skill in type-setting may also, in part, be so explained. The reagent who ranged eighth in the above test belongs, it may be said, to a family of printers. Her father and her mother's brother are both type-setters of unusual skill.

So much by way of suggestion. To return now to a consideration of the records.

Before proceeding with the discussion of the effect of age upon the speed with which mirror-script is read, a word as to the selection and grouping of the reagents. The first group is composed of the nine reagents of my monograph, graduate students or instructors in psychology. These reagents are here referred to by the Roman numerals I., II., III., etc.; they ranged in age from the middle twenties to

the middle thirties. The second group is composed of nine reagents, all juniors in college. This group includes *X* and the students tested with her as control reagents. The members of this group are referred to by the capital letters; they range in age from nineteen years eight months to twenty-four years eight months. The third group is made up of adults of the writer's acquaintance. These reagents are referred to as A_1, A_2, A_3, etc.; they range in age from the middle thirties to seventy years. A last group is composed of nine children from the fifth and sixth grades, sent to me by the supervisor of the training school. These children are referred to as C_1, C_2, C_3, etc.; in age they range from eight years three months to twelve years two months. The youngest child, who has since been promoted to the sixth grade, is obviously a child of unusual ability. The oldest child, C_9, was so careless and indifferent as to make testing him a difficult undertaking.

After the testing of these groups was completed, the second group was enlarged to include, as a whole, 25 seniors and juniors ranging in age from eighteen years ten months to twenty-six years nine months. Furthermore, 25 freshmen girls were tested. These last reagents are referred to by the small letters of the alphabet; they range in age from seventeen years four months to twenty-four years two months. Two sophomores were tested, bringing the whole number up to seventy-nine.

A. Age and Variation in Ability to Read Mirror-script

Stern's and Meyer's results suggest that the varying ability to read mirror-script may, in part at least, and up to an undetermined age, be dependent upon age. In the attempt to determine the facts as to such a possible correlation, the records from the first four groups of subjects were tabulated (see Table I.).

In general, it may be said, the test was not fair to the children, since the sentence used contained words with which the children were unfamiliar. Moreover, the children were unable to profit by the context as did many of the other

TABLE I

RAPIDITY IN READING MIRROR-SCRIPT

(The order within each group is in the order of age, the youngest first. M. and F. indicate the sex of the reagent.)

Group I.
(Graduate Students and Instructors in Psychology)

Reagent	Time
I. (F.)	Over 25 mins.
II. (M.)	1 min. 14 secs.
III. (F.)	1 min. 35 secs.
IV. (M.)	9 mins.
V. (F.)	40 secs.
VI. (M.)	9 mins.
VII. (M.)	7 mins. 30 secs.
VIII. (F.)	16 mins. 51 secs.
IX. (M.)	3 mins. 10 secs.
Average	8 mins. 13 secs.
M.V.	5 mins. 59 secs.

Group III.
(Adults, All Over 34 Years of Age)

Reagent	Time
A_1 (M.)	12 mins. 30 secs.
A_2 (M.)	4 mins. 17 secs.
A_3 (F.)	45 secs.
A_4 (F.)	5 mins.
A_5 (F.)	5 mins. 30 secs.
A_6 (F.)	8 mins.
A_7 (F.)	8 mins. 24 secs.
A_8 (M.)	47 secs.
A_9 (M.)	3 mins. 35 secs.
Average	5 mins. 25 secs.
M.V.	2 mins. 50 secs.

Group II.
(College Juniors)

Reagent	Time
B (M.)	33 secs.
C (F.)	1 min. 10 secs.
D (M.)	37.6 secs.
E (F.)	3 mins. 30 secs.
F (M.)	2 mins. 5 secs.
L (F.)	4 mins. 3 secs.
R (F.)	2 mins. 24 secs.
N (F.)	4 mins. 24 secs.
X (F.)	2 mins. 15 secs.
Average	2 mins. 20 secs.
M.V.	1 min. 6 secs.

Group IV.
(Grade Children)

Reagent	Time
C_1 (M.)	2 mins. 50 secs.
C_2 (M.)	2 mins. 58 secs.
C_3 (M.)	4 mins. 40 secs.
C_4 (F.)	3 mins.
C_5 (M.)	2 mins. 34 secs.
C_6 (F.)	2 mins.
C_7 (F.)	2 mins. 50 secs.
C_8 (F.)	3 mins.
C_9 (M.)	7 mins. 30 secs.
Average	3 mins. 29 secs.
M.V.	1 min. 9 secs.

reagents. Only one child, in fact, seemed to get any meaning from the sentence as a whole. A simpler sentence and one in print instead of script would have been a more legitimate test of the ability of children to interpret such reversals. As, however, the children were the fourth group tested it seemed necessary in spite of the defect in method, to finish with the original sentence. The children, a statement that should be emphasized, showed none of the bewilderment so frequently exhibited by adult reagents on being shown the card. They accepted very simply the statement that the

writing was different from that to which they were accustomed and began reading at once. Most of the letters were recognizable at first sight but only the simpler words were read as wholes.

A survey of Table I. shows at once that if a growth factor is significant, it is cut by very great individual differences. The low records, that is those below 60 seconds, occur in every group except the fourth; one in Group I., two in Group III., two in Group II. With such few reagents and such high M.V.'s the group averages cannot be very significant. It appears, however, that the two groups of younger reagents do show records superior to those of the two groups of older reagents both in the lower group average and the lower M.V. Moreover, if we do not include C_9 in our group of children—his general attitude being as said above very different from that of the other children—we get a group average of 2 minutes 59 seconds with a mean variation of only 25.8 seconds.

The median value of these 36 records is 3 minutes 5 seconds. The following table shows the number of reagents in each group whose speed is above and below the median value.

TABLE II

MEDIAN VALUE 3 MINUTES 5 SECONDS

	Below Median	Above Median
Group I	3	6
Group III	2	7
Group II	6	3
Group IV	7	2
Total	18	18

Such a division also points to the superiority of the two groups of younger subjects. In general the results would seem to indicate that age is one of the factors determining speed of reaction but not the only factor.

On account of the element of unfairness in the test of the children, it does not seem worth while laying stress upon the coefficient of correlation of decreased speed and increased age calculated by a ranking of these thirty-six reagents and

utilization of the formula $R = 1 - (6\Sigma D/(n^2 - 1))$, known as Spearman's footrule. $R = .129$, P.E., .071. Such figures are inconclusive.

Group II., as stated above, was enlarged to include 25 college juniors and seniors. This group is fairly homogeneous. The ages range from 18 years 10 months to 26 years 9 months. It was thought that correlation of speed with lesser age might show for this group as a whole. R, under these conditions, is in fact much higher, since it is equal to .331 with a P.E. of .086. We find, that is, what appears to be satisfactory evidence of a correlation between skill in mirror-reading and lower age. It is interesting to note in this connection that the greatest difference between rank for speed and rank for age is, with one exception, found in the case of reagent X. While, that is, X showed little superiority in absolute speed of mirror-reading, she showed considerable comparative speed if the factor of age be at all significant.

B. Sex and Speed in Reading Mirror-script

If we take the four groups of reagents already discussed, including in the second group the twenty-five college seniors and juniors who were tested, we have in all 52 reagents. Twenty-six of these were girls and women; twenty-six were men and boys. The median record for these 52 reagents is 3 minutes 49 seconds. Table III. gives the distribution above and below the median for the sexes.

TABLE III

MEDIAN TIME 3 MINUTES 49 SECONDS

	Below Median	Above Median
Women and girls	12	14
Men and boys	14	12

There is, that is to say, very little difference in the distribution. If any, it is in favor of the men and boys. The three most rapid records were made by young men.

If we take the 25 college reagents, a more homogeneous group, we find the same sort of distribution. There were thirteen young women, twelve young men. One of the

young women made the median record, 4 minutes 3 seconds. The other reagents fall into groups as shown by Table IV.

TABLE IV

MEDIAN TIME 4 MINUTES 3 SECONDS

	Below Median	Above Median
Women	5	7
Men	7	5

Such a result shows that the sex difference reported where breaking an association between visual and motor factors is concerned does not hold for the interpretation of visual reversals. It would suggest that Whipple's explanation of the superiority of girls and women in the former test as due to their greater familiarity with mirrors is a plausible conjecture.

C. Mirror-reading and Perceptual and Imaginal Predispositions

In this connection we ask what mental factors condition such varying proficiency in mirror-reading. Various factors are, of course, evident. The ability to utilize context was of considerable aid in interpretation.[1] Reagents showing such ability needed to reverse only a fractional part of the sentence. Furthermore, even within the limits of the test there was evidence of visual habituation for certain subjects. These reagents were able to deal with later letters on the basis of their experience with preceding ones, an ability which other subjects did not show, as they repeated the original process of interpretation even on letters occurring several times.

Emotional factors also complicate the situation. In this respect Groups I. and II. had the advantage, since these reagents were well acquainted with the experimenter and, in most cases, had had considerable experience in serving as reagents in psychological tests.

The reagents who read the script most rapidly reported

[1] The utilization of meaning in mirror-reading, tested later by correlation of such a test with results from a completion test, appears to be much less than I should have anticipated, a fact which enhances the value of mirror-reading as a test of specific ability.

that many words were significant as they stood without need of reversal. Subject *B*, who was very expert in mirror-reading and practiced in observation, reports that he notices a curious variation from day to day in the degree to which mirror-reversals appear immediately significant. This report suggests, again, the need of a study of the factor of habituation.

The most plausible conjecture with reference to variation in the speed with which mirror-script is read is that it is dependent upon varying dispositions in the utilization of motor and visual material. Stern conjectured that the child visually preoccupied is more apt to produce visual reversals than the child whose motor activities bring him into intimate contact with space relations. It is natural, then, to ask whether a grouping of reagents as predominatingly visual or motor would have significance in connection with skill or maladroitness in dealing with sensory reversals. So far attempts to answer this question have proved abortive.

The reading of mirror-script was utilized in my monograph on control processes in modified handwriting in the hope of throwing into the foreground certain individual methods of procedure which might be correlated with the control processes used by the same subjects in the writing of mirror-script. A sentence may be quoted from that report to show the general result: "In general, the test had little bearing on the topic under consideration other than showing that the greater mental readiness and facility in adaptation shown by certain subjects in the more distinctly motor tests is paralleled by the result from this test, which is sensory in nature."[1] My conclusion that the striking individual differences evident in the ease with which different reagents did mirror-reading is to be attributed to individual variation in adaptibility was vague in the extreme.

My purpose in the monograph tests was to determine what kind of imagery is used in interpreting and producing mirror-script. Miss Fernald[2] has sought to determine

[1] *Op. cit.*, p. 39.

[2] Fernald, M. R., 'The Diagnosis of Mental Imagery,' Psy. Rev. Mon., 1912, XIV., No. 58.

whether mirror-reading produces a tendency to use any one particular kind of imagery. She says briefly of these tests: "The difficulty or ease did not seem to be correlated with the kind of imagery used." And again: "In general, we may say that this procedure tends to throw the auditory-vocal-motor combination into prominence and to suppress the visual."[1]

One today speaks of imaginal types with considerable diffidence. Not only are we embarrassed in our general conception of sensory predispositions, but also, in the present instance, we do not know whether it is important to lay stress upon the specific method utilized in handling the situation or upon general sensory tendencies. Furthermore, it may well be that the spatial situation in course of development is very different from that found after a mode of reaction has been established. We are, then, thrown back upon necessity of a genetic treatment. Such a treatment lies beyond the scope of the present paper.

A few reports upon the imaginal reactions of the subjects of Groups I. and II. may, however, be included. In all imagery work negative results appear to be particularly significant, where, for example, extensive experimentation with a given subject fails to arouse a specific form of imaginal reaction. Thus, Subject I. (Group I.) as shown by both my work with her and by subsequent tests by Miss Fernald is very defective in visual imagery. She is, also, the poorest, with perhaps one exception, of the seventy-nine reagents whose records I have for the reading of mirror-script.

In Group II., reagents *E* and *F* also show striking deficiencies in visual content. Subject *E*, who employs motor content extensively, is thirty-ninth among the seventy-nine subjects. She was able, however, to utilize meaning to a rather unusual degree. Subject *F*, whose visual bankruptcy is caused probably by extremely poor eyesight, showed considerable skill in deciphering mirror-script but such skill was dependent upon very clever trying-out, in a vocal-motor

[1] *Op. cit.*, p. 47.

way, of one letter after another, and not upon immediate recognition of forms. On the other hand, reagents *C* and *R*, both of whom were good at mirror-reading, show striking preoccupation with visual material in a very great number of situations, *T* being accustomed to translate even auditory content into visual form. *X*, the subject of the report of the first section, also showed strong visual tendencies. *B* had excellent command of a number of imagery forms, including the visual and motor. On the whole, visual preoccupation appears to favor mirror-reading.

§ 4. The Writing of Mirror-script

A consideration of the varying relationship of visual and motor elements in the test under discussion raises naturally the question whether ability to read mirror-script is correlated with ability to write it and the converse. It might be anticipated that there would be no necessary relation between the two, so that a reagent might be much more successful in either the sensory or the motor test. The test to be reported relates, again, only to a first trial and does not, therefore, give evidence as to the important factor of habituation. It was planned to determine the extent of correlation of mirror-script reading and writing, first trial. Two further questions were considered, first, to what extent the ability to read and write mirror-script (first trial) correlates with the ability to read and write inverted script (first trial) and, second, to what extent the mirror-writing of right and left hands correlate.

The reagents for this test were the twenty-five freshmen girls mentioned in the preceding section. These girls were taken from the writer's first-year psychology class and were selected simply by the chance agreement of their free hours with those of the experimenters.

Method.—As a preliminary each reagent was asked whether or not she had practiced the writing of mirror-script, whether she herself showed any left-handed tendencies, whether she possessed any left-handed relatives. The reagent was then timed in the writing, with the right hand, an alpha-

betical sentence of forty letters which had previously been memorized. Secondly, the reagent wrote this same sentence on a second slip, beginning at the right-hand margin of the paper and writing mirror-fashion. Thirdly, the reagent was required to memorize a second alphabetical sentence, also consisting of forty letters and was timed in writing it on a third slip with her left hand, in the normal fashion. Fourthly, the reagent wrote this same sentence with her left hand on slip 4, beginning at the right-hand margin of the paper and writing mirror-fashion. The fifth test was the reading aloud of the sixty-letter sentence written in mirror-script, utilized in the test of the preceding section. Test sixth consisted in writing with the right hand in inverted script on slip 5 the sentence first memorized; test seven consisted in reading a sentence of sixty letters written in inverted script. In every test the speed of reaction was timed.

In the series as planned, the left hand got the benefit of any habituation that occurred from a preceding trial in mirror-writing. There was, also, a slight chance for such habituation to affect the speed of mirror-reading.

Results.—Some general statements may be of interest before an interpretation of particular results is undertaken. The time-range in this group for the mirror-reading (sixty letters) is from 25.4 seconds to 21 minutes 15 seconds; the median value is 3 minutes 23 seconds. The time-range in writing mirror-script (40 letters) is from 45 seconds to 4 minutes 43 seconds; median value, 2 minutes 25 seconds. It would seem, then, that in many cases mirror-writing is less difficult than mirror-reading.

The reading of the sentence written in inverted script (60 letters) ranged in time from 8.2 seconds to 3 minutes 58 seconds; median value, 22.5 seconds. The reading of inverted script is obviously much easier than either the writing or reading of mirror-script. This result agrees with the report of Dearborn that, under conditions of objective reversal, an inversion has less influence upon recognition than any other shift in position.

The speed of writing inverted script ranged from 1 minute

50 seconds to 6 minutes 56 seconds; median time, 3 minutes 13 seconds. Such writing is much more difficult than the reading of inverted script. It is somewhat more difficult than the writing of mirror-script. Again, such a result suggests agreement with Meyer who found in the reproduction of figures that mirror-reversals occurred more frequently than other reversals. It appears, then, that while visually an inversion is more easily handled, the mirror-reversal is more easily produced 'motorly.'

The various coefficients of correlation for the different tests are given in Table V. These coefficients also were calculated by the use of Spearman's footrule for finding *R*. In the first utilization of material, the rank is calculated purely on the basis of speed in writing. Later, the matter of quality of writing will be discussed.

TABLE V

SPEED OF WRITING. RAW CORRELATIONS (*R*)
25 Subjects. P.E. .086

	Reading Mirror-script	Writing Mirror-script, Right Hand	Writing Mirror-script, Left Hand	Reading Inverted Script	Writing Inverted Script, Right Hand	Writing Normal, Left Hand
Reading mirror-script	....	.485	.173	.452	.264	.081
Writing mirror-script, right	.485	....	.413	.456	.490	.062
Writing mirror-script, left	.173	.413	....	.355	.384	.264
Reading inverted script	.452	.456	.346	....	.350	.187
Writing inverted script, right	.264	.490	.375	.350	....	.149
Writing normal, left hand	.081	.062	.264	.187	.149	....

Before proceeding to a discussion of the correlation coefficients, it may be well to recall that, according to Spearman, "*R* has no scientific significance—except negatively—unless it be at least twice as great as its probable error. . . . For a perfectly satisfactory demonstration, the *R* should be not far short of five times greater."[1] Again, "One sees that when a high correlation (say $R = 0.50$) exists between two things, its existence may be satisfactorily proved by—as a minimum—about a dozen cases. But, on the other hand, it is clearly no good attempting to demonstrate a low corre-

[1] *Op. cit.*, p. 96.

lation (say $R = 0.20$) without first collecting for this purpose at least a hundred cases. And, similarly, about 100 cases would be required to prove that no correlation over 0.20 existed. While to prove with certainty the absence of any correlation over 0.02 one would need 10,000 cases."[1]

Inspecting the table with these limitations in mind, we find there is very satisfactory evidence of the correlation of the sensory tests with the motor. R for mirror-reading and mirror-writing, right hand, is .485, a value more than five times its P.E. Reading and writing inverted script correlate less closely, R in this case being only a trifle more than four times its P.E. The two sensory tests (the reading of mirror and of inverted script) show a high correlation, .452; the two motor tests (the right-hand writing of mirror and of inverted script) give an even higher correlation, .490.

These results suggest some interesting questions. The most interesting are those which relate to the left-hand correlations. Thus, mirror-writing with the left hand shows a very low correlation with mirror-reading, although a high correlation with right-hand mirror-writing. A reference to Table V. shows also a fairly satisfactory correlation for left-hand mirror-writing and the reading and writing of inverted script, and also for speed of normal left-hand writing. The reduced and by no means satisfactory correlation of left-hand mirror-writing and mirror-reading may be due to the factor of habituation, since the left hand had the advantage of the right hand practice. Of this, more later.

The questionable correlations of the speed of the left-hand normal writing with either the reading of mirror-script or the writing of it with the right hand is interesting, since according to a traditional view there is thought to exist some relationship between lefthandedness and skill in producing mirror-script.

Another traditional view holds that mirror-writing is the normal writing for the left hand in general. We may ask, then, how mirror-writing with the left hand compares with the normal script produced by that hand and how it com-

[1] *Op. cit.*, p. 97.

pares with right-hand mirror-writing. As before the comparison is first made on the basis of speed only. The time required for writing left-hand mirror-script ranged from 1 minute 5 seconds to 3 minutes 30 seconds, median value, 1 minute 50 seconds. The time required for normal left-hand writing ranged from 25 seconds to 2 minutes 45 seconds, median time, 1 minute 15 seconds. Left-hand mirror-writing is somewhat slower than the unreversed left-hand writing. Left-hand mirror-writing is, however, more rapid than right-hand mirror-writing under the conditions of the test. How far, we may ask, is such increased rapidity due to habituation?

The following facts are pertinent. Six of the reagents, in spite of the habituation factor involved in the second or left-hand test, gave a more rapid record for the right than for the left hand. The other nineteen subjects gave a more rapid record for the left hand. This gain in rapidity varied from 2 minutes 51 seconds to 10 seconds. Reference to the individual records shows, however, that the extreme gain, nearly one and one-half minute more than the next highest, was made by a naturally left-handed reagent, who employs her right hand in writing only because of definite insistence upon its use when she learned to write. Furthermore, the six reagents who stand next in order as showing the greatest gain in rapidity for the left-hand mirror-writing are, with one exception, subjects who reported that they possessed left-handed relatives. The third highest record for increase is, indeed, made by the sister of the left-handed reagent who showed the greatest increase. Possibly, then, these subjects brought to the left-hand mirror-writing not only some practice but also some latent skill in the use of the left as compared with the right hand. The reagents in the middle range show a gain in speed from 10 seconds to 51 seconds, a gain quite within the range of possible habituation. The average increase of the whole group, omitting the one left-handed reagent and the six reagents whose right-hand records were more rapid than their left-hand ones, is 44.6 seconds with a M.V. of 22.7 seconds.

In the monograph tests already referred to, in those cases where the left-hand mirror-script was written first so that the right hand had the habituation advantage, the right hand, although with exceptions, was more rapid than the left.

It does not appear, then, that the left hand is, in general, more proficient than the right in mirror-writing in both right- and left-handed individuals.

Approaching the subject in another way, we may take the reagents in the order of their increased rapidity of left- over right-hand mirror-writing and work out the correlation coefficients (1) for rapidity left-hand mirror-writing and (2) for rapidity normal left-hand writing. We find that *R* in the first case is .024 or practically, 0; in the second case it is .307. That is, the gains in the left-hand mirror-writing correlate not with absolute rapidity in left-hand mirror-writing but, rather, with the rapidity of normal left-hand writing. This would seem to indicate that latent ease in using the left hand enters as a factor determining the speed of habituation.

We find then, that habituation entered into the second mirror-writing test and that this habituation appeared to be greater in the case of reagents with left-handed tendencies. Do these facts account for the lower correlation of left-hand mirror-writing with mirror-reading in comparison with its correlation with right-hand mirror-writing?

Before answering this question, let us compare the mirror-reading skill of the six reagents who—as shown by the fact that their right-hand mirror-writing is more rapid than their left-hand mirror-script—are the most right-handed of the group, with the six who show the greatest gains in the left-hand test. The ranks of the first six for mirror-reading are as follows: 10, 1, 5, 2, 4, 3. The ranks of the second six are as follows: 24 (the left-handed reagent), 15, 23, 25, 11, 16. Such a grouping seems to show that *not habituation merely is responsible for the low correlation of left-hand mirror-writing with mirror-reading but that degree of righthandedness is itself a factor in the high correlation of mirror-reading and right-hand mirror-writing.*[1]

[1] *R* for increased rapidity of left- over right-hand mirror-writing and mirror-reading is −.15. *R* for relative superiority of right-hand mirror-writing and mirror-reading is .394.

Let us now consider these same questions with reference to the quality of writing produced. With such a shift in the standard of comparison we are, of course, introducing a greater chance for error than when using rapidity of work as the basis for ranking reagents. There exists no scale for the rating of left hand or mirror-writing, the available writing scales not being adequate for the measurement of such writing. In order, therefore, to obtain some estimate of the comparative proficiency of different reagents in the quality of their left hand and mirror-writing, five of the experimenters who had assisted in the tests made one serial arrangement each of the twenty-five specimens, for right- and left-hand mirror-writing and for the normal left-hand writing. The slips of paper used were transparent so that it was possible by reversing them to pass judgment upon the mirror-writing under the usual conditions. The average rank and the M.V. for each specimen was then calculated.

The averaged ranks for the left-hand normal writing ranged from 1.2, with a mean variation of .32, to 24.4, M.V., .48. The average M.V. for the twenty-five specimens was 1.92.

The averaged ranks for the left-hand mirror-writing ranged from 2.8, M.V., 1.36, to 24.2, M.V., .64, with an average M.V. for the twenty-five of 2.37.

The averaged ranks for the right-hand mirror-writing ranged from 1.2, M.V., .32, to 24.4, M.V., .96, with an average M.V. for the twenty-five of 2.016.

The qualitative differences for the left-hand normal appear to be more distinct and more easily graded than such differences in the case of mirror-writing with either hand. Right-hand mirror-writing is, however, more easily graded than left-hand mirror-writing.

A third comparison was made, that is, the right- and left-hand mirror-writings of each reagent were compared by three of the judges, chosen so as to include the judge who was least and the one who was most representative of the group as shown by their average mean variation from the average for the group. The judgments of the three judges

were very uniform. Right-hand mirror-writing is, with a few exceptions, of a perceptibly higher grade than is left-hand mirror-writing, in seventeen out of the twenty-five cases. In five other cases the judges agree on the superiority of the left-hand mirror-writing; for three specimens only is there some disagreement as to whether right or left mirror-writing should be marked superior in quality. A reference to the original records shows that the eight subjects forming the second group, whose left-hand mirror-writing excels or equals in quality that of the right hand, are, with two exceptions, those who possess left-handed relatives. The better quality of the right hand mirror-writing would seem to indicate that there is no natural tendency for the left hand to excel in such reversal except in so far as there is a latent tendency to left-handedness.

One asks, further, whether the left-hand mirror-writing is qualitatively superior to the normal left-hand writing as one might expect if the traditional view is correct that mirror-writing is the normal writing for the left hand. The three judges who had compared right- and left-hand mirror-writing also compared the left normal and the left mirror-writing of every reagent. The results were less uniform than in the preceding case. In eleven cases, however, they agreed upon the superiority of the left-hand normal writing and in one case upon the superiority of the left mirror. This last writing was produced by the one left-handed reagent. There were six other cases in which the judges agreed that the left-mirror was equal to or a trifle superior to the left normal. In the six remaining cases there was disagreement, with two of the judges agreeing in four of these cases that the left normal was superior to the left mirror. So far as the evidence goes it seems to point to a more frequent superiority of the left-hand normal although this superiority is less common than is the superiority of the right-hand mirror-writing.

Coefficients of correlation with a ranking based on qualitative differences were worked out and are given in Table VI. The chances for error are, of course, very great. Even so, the figures are suggestive.

TABLE VI

QUALITY OF WRITING. RAW CORRELATIONS (*R*)

	Quality			Speed			
	Mirror-script, Right	Mirror-script, Left	Normal Script, Left	Mirror-script, Right	Mirror-script, Left	Normal Script, Left	Mirror Reading
Right mirror	.....	.524	.225	.062	.....	.....	.278
Left mirror	.524	.....	.370	.....	−.129	.....	.149
Left normal	.225	.370	.....	.....	.....	−.048	−.096

From this table we see that on the basis of quality, as well as of speed, right-hand mirror-writing correlates more closely with mirror-reading than does left-hand mirror-writing. The quality of left-hand normal writing does not correlate with mirror-reading. In general, the lack of correlation of speed and quality is instructive and should make possible interesting comparisons if we had records from the subjects as to kind of sensory control utilized in the test.

§ 5. SUMMARY AND CONCLUSIONS

The first section of this paper reports a case of spontaneous right-hand mirror-writing which presents points of interest in that even in the adult state the subject shows a tendency to revert to this form of writing and gives evidence of maintaining mirror-reversals in her imaginal representation of words. The mirror-writing seemed conditioned by a general difficulty in orientation which from the social side showed itself in some inadequacy in dealing with the meaning-aspect of experience. Practically, the reagent found herself skilful in type-setting with subcapacity in the interpretation of varied form, such as that found in illegible writing.

The use of other reagents in a control series of tests (section three) to find the reagent's comparative skill in interpreting mirror-reversals showed that there were college students who excelled *X* and revealed striking individual differences in the ability to deal with such reversals. The literature of the subject (section two) suggested that possibly age might be the controlling factor in such varying capacity since spatial position appears to be an outcome of the fusion of the visual and motor elements of experience. The evidence

at hand points strongly to growth as one of the determining factors but indicates, also, the presence of other factors. Sex did not appear to be one of these factors. It seems plausible as a conjecture that capacity to interpret mirror-reversals is dependent upon visual as against a motor preoccupation.

A fourth section of the paper sought to determine the range of variation in skill in mirror-writing among twenty-five freshmen and to determine how far such skill correlated with skill in mirror-reading. The correlation was found to be high with the right hand, much higher than with the left. Apparently the results, in contradiction to the traditional view, point to a correlation of degree of right-handedness (or, possibly, degree of specialization of function) and efficiency in mirror-reading. This is what one might expect if specialization of function goes with increased adequacy in orientation.

But have we any reason to believe that efficiency in mirror-reading is in any way related to adequacy of spatial orientation in general? We are left, moreover, with certain puzzling questions on our hands. How account for decrease in skill in interpretation of spatial reversals with age if such skill be correlated to any degree with increased adequacy in orientation? How reconcile the conjecture that efficiency in mirror-reading may be due to visual preoccupation with the apparent correlation of such efficiency with right-handedness?

Obviously, the tests just reported are insufficient to answer these questions. More extensive tests are called for. Specifically, one should utilize the various tests of the index of righthandedness. One should determine whether such a thing as degree of righthandedness correlates with age. If possible, some classification of reagents into visual and motor should be attempted. Above all, the effect of habituation should be studied for quite possibly this may be found to result differently in the visual test (mirror-reading) from what it does in the motor test (mirror-writing).

Certain of these tests the writer hopes to carry to some conclusion.[1]

[1] I wish to express my thanks to the Misses Foster, Eby, and Johnson, and to Mr. John Hill and Mr. John E. Anderson for their aid in gathering data for the above report.

A COMPARATIVE STUDY OF RECOGNITION AND RECALL[1]

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During the last several years McDougall,[2] Calkins,[3] Strong,[4] Hollingworth[5] and others have pointed out the fact that there is a decided difference between recognition and recall memory. The attempts to measure this difference, however, have been rather meager.

The following study was started in the winter of 1910–11 when the writer was gathering data for his 'Study in Incidental Memory.' 109 boys and 125 girls of Royersford, Pa., from the high school to the fourth grade inclusive had been asked to spell some words. Six words were pronounced as for a spelling test and after one half hour the subjects were unexpectedly asked to recall the words. After 3 months the subjects were again surprised by a request for a second recall. After two minutes had been given for recall, and papers had been collected the writer pronounced the six words, distributed by chance, among 10 other words. The children wrote these words as they were pronounced and were then asked to mark the words they recognized.

To the surprise of the writer, while not one of the 234 recalled 6 words and 55 recalled none, only 1 failed outright to recognize any of the 6 words, and 62 subjects,—32 boys and 30 girls recognized all 6. Of these 32 boys and 30 girls who recognized all 6, 23 boys and 25 girls had no wrong ones.

[1] Reported before the Conference on Individual Psychology held at Columbia University, April 6–8, 1914.

[2] McDougall, R., 'Recognition and Recall,' *J. Phil., Psychol. and Sc. Methods*, 1904, Vol. 1, pp. 229–233.

[3] Calkin, Mary Whiton, 'A First Book in Psychology,' Ch. VIII., pp. 124–132.

[4] Strong, Edward K., Jr., 'The Efficiency of Length of Series Upon Recognition Memory,' PSYCHOL. REV., 1912, Vol. 19, pp. 447–462.

[5] Hollingworth, H. L., 'Characteristic Difference between Recall and Recognition,' *Amer. Jour. Psychol.*, 1913, Vol. 24, pp. 533–544.

In as much as those who recognized all six words were not measured to the extent of their ability, and therefore since the total percentages for recognition were naturally too low, the writer did not venture to include the records in the 'Study in Incidental Memory.' The works of Hollingworth,[5] and Strong[4] which have appeared in the meantime stimulated a continuation of the study.

Having tested with 10 words 350 children of the public schools of Tyrone, Pa., on the influence of recall in retention, about a year ago, the writer returned ten months later for a recall and recognition test. He gave the 10 words which had been previously given along with 10 other words for the recognition test.

It is obvious here that because of the nature of the tests the same experimenter can test only a limited number in a place, for no valid tests in incidental memory can be made where the subjects have a knowledge of the nature of the test beforehand.

With 20 words as stimuli and with these words distributed by chance among 20 other words, tests were made in the public schools of three other towns (Mt. Union, Bridgeport, and Conshohocken, Pa.). For one town (Tyrone) one week intervened between the presentation of the stimuli and the first and only recall and recognition. In the two neighboring towns (Bridgeport and Conshohocken) only one day intervened. In these three schools Mr. Raymond Ellis a junior in the writer's class in psychology in Juniata College was experimenter. His acquaintance with the respective superintendents and many of the teachers contributed toward the success of the experiment.

The superintendents, teachers and experimenter were provided with printed copies of the directions. The experimenter pronounced the stimuli words and the words for recognition. Each word was pronounced only once and the speed was determined by the time when the pupils looked from their papers. The teachers and superintendent aided in giving the directions and in the prevention of cheating. The teachers as well as the children, however, were left

under the impression that it was a mere spelling test. In all, 687 subjects,—333 boys and 355 girls were tested.

Since the same subjects were tested and with the same materials for recognition and recall, and since the recall test was always given before the recognition test, two assumptions were obvious, namely, that any word which could be recalled could always be recognized afterwards and that no word could be recalled which could not first have been recognized. A study of the Royersford group shows that the first assumption is well grounded, for only three of the 234 subjects recalled words (one word each) which they did not afterwards score as having recognized. The probability is that these 3 merely failed to see these words when they checked up for recognition, due to haste or emotional disturbance. This fact is equally obvious in the other groups. No way presented itself in this study to readily test the second assumption.

Total averages for recall, and for recognition without regard to chance, were computed in terms of words and per cent. The comparison emphasized most is between recall and efficiency of recognition. The latter was computed from the averages of all the individual percentages by Strong's formula[4] $C/T \times ((C - W)/(C + W)) \times 100$ per cent., for by all the groups but one in the words for recognition, since the same number of stimuli words as other words obtained, as many correct as incorrect words would be marked by mere chance. For the group where ten words were added to the six stimuli words this formula was modified thus: $C/T \times ((C - W \times .6)/(C + W \times .6)) \times 100$ per cent. A comparative study was also made of individual records, as well as a study of the correlation between recall and recognition. Likewise tables of frequency for recall and recognition, along with the average deviation from the averages were computed.

Table I. gives the total averages by the four groups tested for recall and recognition in terms of words and per cent., the ratio of recognition efficiency to recall, and the average group-gain of recognition over recall. Beginning at the left one reads:—"males of B. and C. schools on the average

recalled 4.5 words or 22.9 per cent. of the whole number of (20) words given, with a variation from the average of 8.9 per cent. The same group have an average efficiency for recognition (according to above formula) of 66.5 per cent. with an average deviation from the average of 14.6 per cent. On the average, without considering those scored for recognition by mere chance, 16.5 words were recognized or 82.5 per cent., with an average deviation of 10.7 per cent. The ratio of recognition efficiency over recall is 2.9. The gain of average recognition efficiency over recall is, for this group, 43.6, and 12 more words were recognized on the average than were recalled." As would be expected the highest averages for recall and recognition obtained after one day's interval and the lowest after ten months, with a considerable gain after one day, over recall after one week. However, it must be remembered that the Tyrone group (ten months' interval) was given only ten words while the one-day and one-week groups were given these same ten words with ten more, and the three week group had been given only six words. Therefore a fair comparison for effect of time interval can be made only between the first two groups. The variability for recall runs from 6 per cent. to 19 per cent. or .6 to 1.9 words and is decidedly less after one week than after one day. The highest variability obtains for the three week recall for six words.

The average per cent. for efficiency in recognition and for total correct recognition take the same general trend as those for recall. However, the variability is almost constant for efficiency of recognition, save for the Royersford group where it is proportionally high. The variability fluctuates somewhat more for total correct recognition, but the total variability for Royersford is not proportionally high. While the absolute variability for recognition is considerably higher than that of recall the relative variability in terms of the respective amounts recalled and recognized is much lower for recognition than for recall. The gain of recognition over recall in per cent. and in words is shown in the last two columns. They show a higher increase for one day recall than for one week.

With no regard for records by chance, the individual gains of total recognition over recall are shown in the following tables of frequency:

GAIN OF TOTAL RECOGNITION OVER RECALL IN WORDS

No. Words	B. and C. (20)		Mt. Union (20)		Tyrone (10)		Royersford (6)	
	1 Day		1 Week		10 Months		3 Weeks	
	M.	F.	M.	F.	M.	F.	M.	F.
17	7	1	....	1	....	....	....	...
16	6	2	2	3	....	....	....	...
15	11	4	7	3	....	....	....	...
14	11	9	3	8	....	....	....	...
13	19	20	7	8	....	....	....	...
12	25	21	6	3	....	....	....	...
11	15	16	3	10	....	....	....	...
10	6	17	6	7	....	....	....	...
9	9	8	2	6	1	1	....	...
8	7	6	4	7	3	2	....	...
7	2	3	1	2	17	11	....	...
6	2	3	....	2	14	16	2	4
5	2	....	3	2	6	9	17	7
4	....	....	5	2	7	7	24	15
3	....	....	1	....	2	7	29	44
2	2	....	....	....	....	2	24	27
1	....	....	....	....	....	....	9	7
0	....	....	....	....	....	....	4	18
.....	....	....	....	....	....	....	....	3
Ave....	12.0	11.3	10.6	10.9	6.0	5.4	3.1	2.5

The figures in parenthesis indicate the number of stimuli-words used. Under "M." of Mt. Union one reads, for example, "Two boys scored 16 more words in recognition than they recalled; seven, 15 more; three, 14 more, etc. The gain of total recognition over recall is about one half the number of stimuli-words used for the respective groups. A study of the B. and C. group shows a slight increase in average gain of total recognition over recall, from the lowest grade to the high school. By the boys the gains are, 10.8 words for the 5th and 6th grades; 11.4, for the 7th and 8th grades; and 14.2 for the high school. For the girls, the records are 10.8, 11.0 and 15.6 words respectively. On the other hand, the Mt. Union group has the lowest gain for the 7th and 8th grades, and the lowest grades are about as high in their gain as the highest. In order of the figures for the

B. and C. groups, the respective gains for the boys by Mt. Union are, 10.8, 9.7, and 11.1 words; for the girls, 11.4, 9.5, 11.9 words, respectively. Out of 105 cases tested after 10 months, 103 cases had more correct than incorrect answers for recognition, and therefore, made more than a zero record in recognition.

For all the groups the girls greatly excel the boys for recall. There is a small superiority of girls over boys for total correct recognition, but a slight exception obtains for this rule of sex difference in efficiency of recognition: the ratio of recognition to recall, the gain, by the girls, of per cent. recognition efficiency over recall, and of average number of words correctly recognized over those recalled show the boys strikingly superior to the girls. The same holds true for all groups throughout the grades (Table II, not printed). This indicates that while the girls are a little superior to the boys in proportion to their efficiency in recall they are inferior to boys in recognition. For recall the girls show more variability than boys, while for recognition the opposite is true.

A comparison by grades Table II. reveals the same superiority of girls, for recall for every grade of every group. However, no rule holds true for sex difference for recognition efficiency nor can any rule for sex difference for variability be formulated for recall or recognition. Furthermore the greater gain by the boys than by the girls in recognition over recall holds true by grades almost without exception, as a little subtraction in Table III. will show.

Table III. contains the tables of frequency of the groups by grades combined for correct and for incorrect words recalled and recognized. For example one reads from first column to the left under "M." of B. and C.: 2 subjects recalled 12 words, 1, 11 words; 1, 10 words; 3, 9 words; 4, 8 words, etc. At the foot of each column is the average number of words recalled and recognized. These averages show girls superior to boys for recall, as one can readily see also by frequency table; but for incorrect recall the girls stand slightly higher while in all the groups the boys recognize more wrong words than girls. There is a general increase

for both sexes in incorrect answers and decrease of correct answers with increased period of time.

Table IV. gives the percentages of all answers given that are correct. With the exception of the group with one day interval more answers are correct for recognition than recall and the superiority of the recognition answers over those of recall increases with increase of time. It would seem that one's recognition memory after a long interval of time is much more reliable than one's recall memory.

	Mt. Union				Tyrone			
	Time 3 Wks.: 20 Stimuli-words				Time 10 Mo.; 10 Stimuli-words			
	Recall		Recognition		Recall		Recognition	
	Correct	Incorrect	Correct	Incorrect	Correct	Incorrect	Correct	Incorrect
1	5	0	15	1	1	1	6	4
2	1	4	16	4	1	0	8	2
3	0	0	15	6	0	0	9	1
4	1	0	17	8	1	2	5	3
5	4	1	17	4	0	3	7	2
6	4	4	16	2	0	2	3	3
7	3	0	15	4	1	1	8	2
8	3	1	17	3	1	0	5	2
9	1	0	11	2	1	5	5	5
10	1	1	13	1	0	1	7	3
11	3	1	11	1	1	1	6	4
12	1	1	16	10	2	4	8	2
13	3	0	14	8	3	2	7	2
14	4	0	17	5	2	1	7	1
15	5	0	18	2	3	1	6	3
16	4	1	13	3	4	4	6	4
17	3	1	18	5	0	0	4	1
18	3	1	16	2	2	2	9	1
19	2	1	10	0	2	1	6	4
20	5	5	12	1	0	4	7	3

A correlation between recall and recognition by the Royersford group gives, by Pearson's method, for the boys, .596; girls, .304. By the method of unlike signs the correlations are .535 and .308 respectively. Since these two methods gave such like results the latter method only was used for the other groups. For the boys in the B. and C. group the figures are +.209, for the girls .517 and in the Mt. Union group they are −.209 and 0 for the boys and girls respectively, for Tyrone the correlations are .011 and .468. Aside from the Royersford group where such a large

number made perfect records in recognition, the correlation for the girls is superior to that for the boys. All the correlations are remarkably low for all groups and for one group is practically absent. Perhaps these figures reveal a bigger difference between recognition and recall memory than any other figures.

Twenty records selected at random from each of the Mt. Union and Tyrone groups are given above.

It is an interesting fact that an individual who recalled no words recognized 15 and another who recalled 5 recognized only 15. Random sampling from any of the groups show many examples as striking as these. It is evident that what has apparently been forgotten outright is really clinging somewhere in the field of consciousness.

As pointed out by Kirkpatrick[1] and by Myers,[2] many of the incorrect words given show a high degree of association in form or in meaning, with the stimuli-words. This holds true for both recall and recognition, and for all the groups with the various time-interval and stimuli. General ideas which seem to have been called up when the words were given are evidently carried over. In the Royersford group most of the stimuli words (though not purposely so) refer to a house or household effects and are especially suggestive of kitchen and attendant associations. They are: angel, pickle, dirt, busy, onion, women. As a result, both for this group and for the Tyrone grades, tested with these same six words, along with four more stimuli-words, one finds that among the incorrect words 64 per cent. of the answers seem to be associated with a house in general, with edibles, or with culinary objects. The following are some suggestive words of this type, taken from the combined records of the above-named groups:

7 dish	4 basket	3 coal	1 dust
6 bucket	4 pitcher	2 stove	
5 kitchen	4 broom	2 pan	

[1] Kirkpatrick, E. A. An Experimental Study of Memory, Psychol. Rev., 1894, 1, pp. 602–609.

[2] Myers, G. C. A Study in Incidental Memory, Arch. of Psychol., No. 26, 1913, pp. 64–65.

The figures to the left indicate the number of subjects who gave the respective words. Four subjects gave "wagon," 3 of whom gave it along with "tongue," and 11 gave words with "oo" in them. It is worthy of note that none of the incorrect words are long, but they tend to approach the length of the stimuli-words. No sex difference obtains save that 6 girls named "dish" while only one boy named it, and 4 girls and no boys named "pitcher" among their incorrect recall. On page 64 of "Incidental Memory" the writer has given a list of generally suggestive words, selected at random, for the Royersford group. A complete list is also given for the same group after an interval of one-half hour.

In the group tested after one week, for recall of ten words, the following significant terms were given:

5 horse (donkey)	1 lead (iron)
4 sugar (grocer, pickle, or lemon)	3 candle (lantern)
4 flower (blossom)	2 string (spool, grocer)
3 garden (dirt, shovel)	4 school (spool)
3 mineral (iron)	3 apple (pickle, kettle)
2 copper (iron)	3 monkey (donkey, money)

Likewise in recognition certain words prove to be similar in form or meaning.

For example "picture" which stood first in the list presented for recognition test, was mistaken for "pickle" (obviously) by 87 out of 105 pupils.

After ten months the comparative order of frequency for each word by recall and recognition is shown by the following:

Recall	Recognition	Original Order (correct)
29 shovel	90 shovel	shovel
20 kettle	88 kettle	tongue
18 onion	79 onion	spool
17 spool	77 grocer	kettle
16 pickle	77 spool	feather
11 clock	72 dirt	clock
8 dirt	71 tongue	pickle
4 tongue	67 pickle	dirt
3 feather	64 feather	grocer
2 grocer	43 clock	onion

It might be noted that in the test on the Tyrone group the writer by mistake told the subjects to recall "as many

of the *ten* words," which he had pronounced for them about ten months before as they could. In all the other tests the number of words was not indicated. Three fourths (72 per cent.) of the Tyrone group marked just ten words; while for about the same number of subjects tested with twenty words after three weeks, only 13 per cent. marked the correct number of (20) words as recognized. This indicates that the recognition records for the Tyrone schools are proportionally higher than the recognition for the other groups.

The writer suggests that a study of the effect upon the subjects of their knowing the amount of stimuli to be recalled or recognized would be worthy of a careful study: for example if one has to go shopping for several articles a memory of the number of articles to be gotten will help assure none being missed. This gives one a general scheme and a definite destination in his struggle to remember, and surely in recognition it will tend to make one weigh more carefully the materials from which the familiar stimuli are to be selected.

To the most casual observer the affective element is tremendously more expressive in the process of recognition than in that of recall. It was necessary in all the recognition tests to warn the subjects against uttering exclamations when the word they chanced to recognize was pronounced, for many wanted to laugh aloud or make various types of happy expressions when such words were pronounced. In the movement of limbs and body, and especially in the smiles and facial expression one could almost determine how many words in the recognition list were really familiar to each individual. Certainly the affective side of recognition has not been duly emphasized.

Calkins,[3] Allin,[1] and a number of others observe that a feeling of pleasure generally or always attends recognition, but they give to this phenomenon of recognition only a minor place. The writer maintains that the pleasurable feeling frequently and perhaps always, precedes conscious recognition. A horse sold by his master to a neighbor, will neigh for years when passing the old barn, and will look toward his old home with movements that indicate pleasure.

[1] Allin, Arthur, Recognition, A. J. Psychol., 1895–6, Vol. 7, pp. 240–373.

The dog will make unmistakable movements of happiness when an old friend of his returns home. Certainly no conscious association of ideas is here; yet none would doubt the expressed feeling of recognition. The writer's baby at five months of age after being absent for a day or two from the room and bed to which he was accustomed, kicked and smiled and made extraordinarily numerous movements when again put on his bed.

The writer may not be a good subject but a number of records from his own experience seem to emphasize this affective factor of recognition. Following are a few of these records:

I met a Mr. M. in town H. at Christmas time. At first sight I had a feeling of acquaintance, and at various times since, on referring to him or on hearing his name mentioned I experienced the same feeling: I had no consciousness of any other individual nor did I once have the feeling "He reminds me of some one." On the evening of May 20 following, M.'s image suddenly came to me and with it the image of Mr. F. Then I discovered at once in F. the source of this long-continued feeling.

A certain student (Miss L.) was first in my class February 2. The feeling I had on seeing her then and on almost every day following when I saw her in the class, was one of "at-homeness": but not once did it occur to me that she reminded me of Miss M. However on June 11 of the same year I caught a faint glimpse of a lady on the street, who called up the Miss M. Immediately Miss L.'s image came to me and I had discovered the cause of the feeling. It was the resemblance of Miss L. to Miss M.

In all these cases the feeling persistently antedated the association of ideas. However, one might explain this in terms of "subliminal association," or the "subconscious."

Whatever is the prime cause, the feeling in such cases surely comes to consciousness first. Therefore, as far as one's knowing about it is concerned one's recognition is primarily a feeling.

From this study the following conclusions are derived.

1. The recognition efficiency is about two and one half

times that of recall, and this ratio varies slightly with different amounts of stimuli and with different intervals of time. However, the total number of words correctly scored in recognition was about four times the number recalled. The difference in ratios is due to the penalty assigned because of chance.

2. Great individual difference obtains for both recognition and recall, but for recognition it was proportionally higher than for recall, and difference of time intervals and length of stimuli-list affect the variability of recall more than that of recognition.

3. The correlation between recall and recognition is surprisingly low: many who recall only one or two words or even none have a remarkably high record for recognition.

4. There is a general increase of incorrect answers and a decrease of correct answers with an increase of time-interval.

5. For long intervals of time more of the answers for recognition are correct than those for recall and this superiority of recognition-answers increases with increase of time interval.

6. Many of the incorrect words given show a high degree of association in form or in meaning with the stimuli-words; general ideas are carried over most frequently.

7. The comparative order of frequency for each word, in recognition and recall is about the same for the first words of the stimuli-list, but there is a wide variation for those least frequently recalled.

8. A knowledge of the number of words to be recognized seems to be a great aid in recognition.

9. The affective element is very much more pronounced in recognition than in recall.

10. The wide difference in results obtained from the different groups under different conditions show how unreliable it is to derive general laws from small groups of subjects arbitrarily tested. Therefore the conclusions herein derived are necessarily limited to the tests described in this study, until they shall have been verified by further studies.

11. Some sex differences are obvious:

(*a*) The girls are superior to the boys for both recognition and recall, and much more for recall than for recognition. Their superiority for recall invariably holds true even when divided into grades, while for recognition this superiority obtains only for large groups.

(*b*) The most striking sex difference is shown in the ratio of recognition to recall; while on the average, the boys' efficiency for recognition is three times that of recall, the ratio by the girls is about two. Likewise the total gain in per cent. in recognition efficiency over that of recall is markedly superior for the boys.

(*c*) A higher correlation between recall and recognition obtains for girls than for boys.

(*d*) For recall the girls seem to be more variable than boys while for recognition the opposite holds true.

(*e*) The girls recall more incorrect words than the boys, while the boys recognize more incorrect words than the girls.

(*f*) Both for recall and recognition a higher percentage of the total answers given by the girls than those given by the boys are correct.

TABLE I

TOTAL AVERAGES

No. Subjects	Recall			Recognition					Ratio of Recognition Efficiency to Recall	Gain of Average Recognition Efficiency Over Recall in Per Cent.	Gain of Total Recognition Over Recall in Words
		Efficiency			Total Correct						
	Words	Per Cent.		Per Cent.		Words	Per Cent.				
		Ave.	A. D.	Ave.	A. D.		Ave.	A. D.			
Bridgeport and Conshohocken (time 1 day)											
M. 124	4.5	22.9	8.9	66.5	14.6	16.5	82.5	10.7	2.90	43.6	12.0
F. 110	5.9	29.4	9.9	70.5	14.5	17.3	86.6	8.4	2.39	41.1	11.3
Mt. Union (time 1 week)											
M. 50	2.3	11.5	6.5	37.2	14.2	12.9	64.3	19.7	3.23	25.7	10.6
F. 64	3.5	17.5	5.9	39.9	13.2	13.6	68.1	16.5	2.28	22.4	10.9
Tyrone (time 10 mo.)											
M. 50	.9	8.6	6.9	29.9	15.2	6.9	68.6	10.2	3.48	21.3	6.0
F. 55	1.5	15.0	12.1	28.7	14.9	6.9	68.9	10.5	1.91	13.7	5.4
Royersford (3 weeks)											
M. 109	1.5	25.5	17.3	60.7	23.9	4.5	75.1	18.7	2.38	35.2	3.1
F. 125	2.0	33.9	19.2	63.0	19.2	4.6	76.0	15.9	1.86	29.1	2.5

TABLE III

TABLE OF FREQUENCY

No.	Correct												Incorrect											
	Recall						Recognition						Recall						Recognition					
	B. and C.		Mt. Union		Tyrone		B. and C.		Mt. Union		Tyrone		B. and C.		Mt. Union		Tyrone		B. and C.		Mt. Union		Tyrone	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
.....	124	110	50	64	50	55	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
20	.....	.....	.....	.....	.....	.....	11	14	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
19	.....	.....	.....	.....	.....	.....	23	25	2	4	....	....	....	....	....	....	....	....	....	....	....	....	....	....
18	.....	.....	.....	.....	.....	.....	19	20	1	6	....	....	....	....	....	....	....	....	....	....	....	....	....	....
17	.....	.....	.....	.....	.....	.....	16	17	6	11	....	....	....	....	....	....	....	....	....	....	....	....	....	....
16	.....	.....	.....	.....	.....	.....	21	14	6	2	....	....	....	....	....	....	....	....	....	....	....	....	....	....
15	.....	.....	.....	.....	.....	.....	9	8	5	5	....	....	....	....	....	....	....	....	....	....	....	....	....	....
14	.....	.....	.....	.....	.....	.....	10	5	6	11	....	....	....	....	....	....	....	....	....	....	....	....	....	....
13	.....	.....	.....	.....	.....	.....	5	5	2	6	....	....	....	....	....	....	....	....	....	....	1	1	....	....
12	2	3	.....	.....	.....	.....	2	1	3	4	....	....	....	....	....	....	....	....	....	....	....	....	....	....
11	1	4	.....	.....	.....	.....	3	....	6	3	....	....	....	....	....	....	....	....	....	....	....	2	....	....
10	1	4	.....	.....	.....	.....	3	1	3	5	....	....	....	....	....	....	....	....	....	....	2	....	....	....
9	3	3	.....	.....	.....	.....	....	....	2	2	5	8	....	....	....	....	....	....	1	....	2	2	....	....
8	4	10	.....	.....	.....	.....	1	....	2	1	12	10	....	....	....	....	....	....	1	....	4	2	....	....
7	9	15	2	3	.....	.....	....	....	3	....	13	20	....	....	....	1	....	1	....	1	....	5	....	....
6	15	21	.....	3	.....	.....	....	....	2	....	13	11	....	....	2	1	1	....	3	1	6	6	....	....
5	18	18	1	7	.....	1	....	....	1	1	5	3	1	....	2	3	3	....	5	4	6	6	....	....
4	29	14	6	17	.....	4	1	....	....	....	2	3	....	....	4	3	2	7	14	12	9	7	4	....
3	19	11	12	19	.....	10	....	....	....	....	....	....	8	3	2	8	4	6	17	19	4	8	13	9
2	15	4	7	11	13	12	....	....	....	....	....	....	4	4	7	7	10	13	27	21	6	9	13	22
1	5	2	20	3	17	10	....	....	....	....	....	....	21	18	12	22	11	17	29	32	8	6	14	15
0	3	1	2	1	20	18	....	....	....	3	....	....	90	85	21	19	19	11	27	20	2	10	6	9
Average..	4.5	5.9	2.3	3.5	.9	1.5	16.5	17.3	12.9	13.6	6.9	6.9	.5	.3	1.4	1.7	1.4	1.7	2.0	1.9	5.2	3.8	2.9	2.6

Table IV

Per Cent. Correct of All Answers Given

	Recall	Recognition
	B. & C.	
M. 124	90.5	89.1
F. 110	94.8	90.0
	Mt. Union	
M. 50	61.7	74.7
F. 64	69.1	78.0
	Tyrone	
M. 50	37.4	70.3
F. 64	46.9	72.9
	Royersford	
M. 109	75.2	81.0
F. 125	76.3	86.9

THE AUTOMATIC WRITING OF CHILDREN FROM TWO TO SIX YEARS, INDICATIVE OF ORGANIC DERIVATION OF WRITING IN GENERAL

BY ANNA WYCZOLKOWSKA

It was on a steamer going from New York to Hamburg, in the year 1908, that my attention was called for the first time to spontaneous writing by children. I noticed a little girl, three years, and her brother two years of age, scribbling

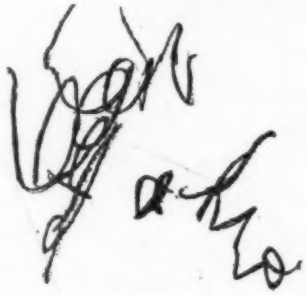

FIG. 1

FIG. 2

various lines on a piece of paper. Having become acquianted with the little company, I could enter into possession of some of these graphical specimens reproduced in Figs. 1, 2 and 3.

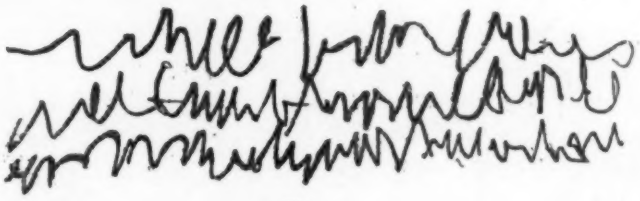

FIG. 3

We see here two different results of automatic movements of the hand projected on paper. Figs. 1 and 2 show delicate and incoherent lines produced by a child two years old, which

starts such exhibit of points and lines with an obvious hesitation and timidity; Fig. 3 on the contrary represents a

This figure was taken recently by a little girl of two years of age, under the suggestion: "Write a letter to Santa Claus and he will send you a present."

FIG. 4

graphical complication unexpected from a child three years of age.

When I returned to Chicago I started a systematic study

of automatic writing by children with a little girl two and half years of age, which observations have been made through two consecutive years, after which time the child began to imitate printed or written letters of adults; consequently was passing from automatic to assimilated writing.

Aldona, who was previously the subject of my observations in reference to speech, had shown during all this time such an inclination to spontaneous writing, that no table or wall, no chair or book was spared from her graphic exhibit, moreover I had a continual insight into the gradual development of her graphic dispositions. When these tests with Aldona were commenced she had already begun to scribble by the projection of mechanic movements of the hand from the right to the left side, limited only by the edges of the paper or blackboard. This probably is the reason why I never could observe in her the earliest graphical signs of children, reproduced in Figs. 1, 2 and 4. Observing her scribbling, produced with much energy and continuous impulses, I noticed that her attention was mostly stimulated by the pleasure in moving the hand and not by the result obtained on the paper.

After a few weeks I could observe a certain change or rather a progress in the graphic capacity of Aldona, when she produced more attentively some concentric, horizontal and vertical lines. The latter especially were characteristic on account of the waving form.—When Aldona was three years and three months old, her mother promised her to arrange a party for her little friends. I took the opportunity to suggest to the child that she write personal letters of invitation. She understood the point and when I handed her a sheet of paper and a pencil, she accepted them and, after short hesitation, asked me: "What am I to write?" "Invite them to the party and say that you will have a good time together" was my answer. Upon this suggestion her hand begun to move quickly across the paper, interrupted only by its edges, and in a few minutes I obtained a new specimen of her spontaneous writing, Fig. 5. The characteristic difference of this writing with that of the previous stage

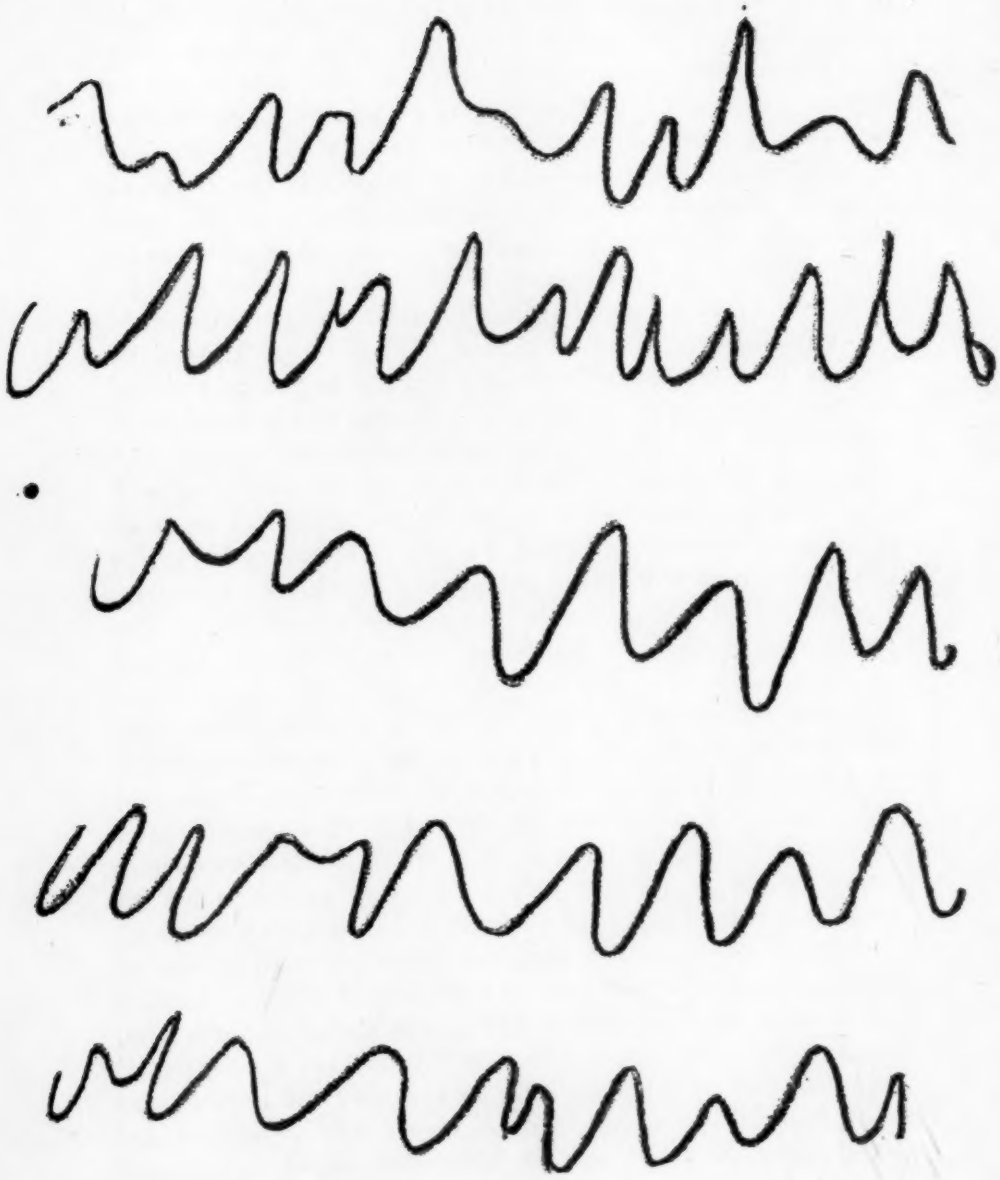

FIG. 5

consists of a considerable diminution of the waving line and the change of relations between the amplitude and the phases of the curve: the first are now higher, the second much shorter. Secondly, I observed a progress in the intensity of attention by this kind of writing. Having finished the letter the child grasps the envelope ready to write the address. In the meantime I remembered two letters received previously, by the mother of Aldona from her niece Dorothy,

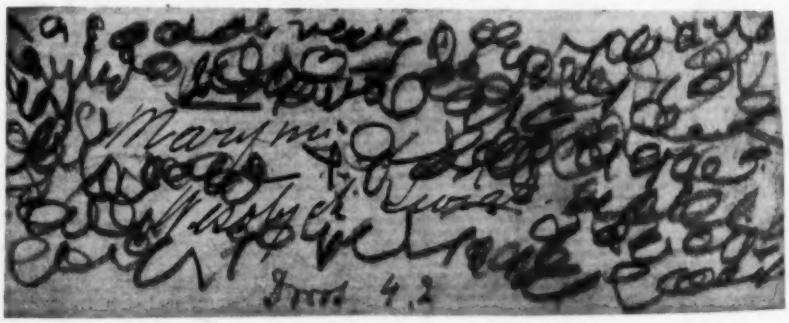

FIG. 6

two years older than her own daughter. These two letters reproduced in Figs. 6 and 7, as the most interesting specimens of spontaneous writing by children, show no affinity to the previous letters of Aldona. Both letters of Dorothy are of a more artificial structure, and the second is especially characterized by an important phenomenon, namely, that many letters of the alphabet of different languages like German, Greek, Latin and even Arab or Sanscrit can be easily detected in this stage of automatic writing evolution, which detail will be explained later.

The question became more interesting but at the same time more difficult to explain, because these two kinds of letters were not only different in their character but even were contrasting each other, the first being uniform and automatic, the second complicated and artificial.

A new stage in the graphical development of Aldona has thrown a new light concerning the multiplicity of this phenomenon. When Aldona was four years old she abandoned

Fig. 7

FIG. 8

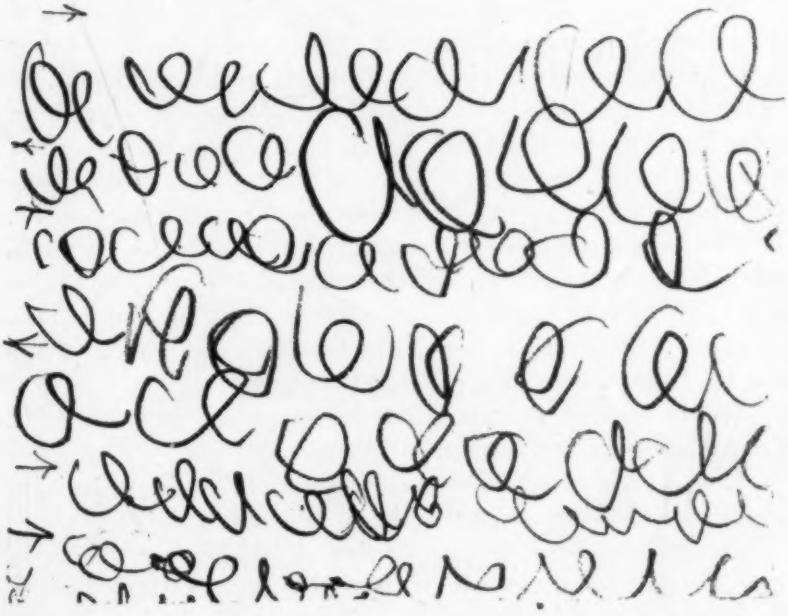

FIG. 9

her recent habit of writing continuous curves, changing it to a kind of combined zigzags, more or less connected with each other (Fig. 8). If we compare this new specimen of her writing, as well as the other specimen *very similar*, taken recently by a little girl, Alicia, four years and eight months old (Fig. 9), with the first letter of Dorothy (Fig. 6), we become aware of the affinity between all of them. They differ only so far in this respect that the writing of Aldona, Fig. 8, and Alicia, Fig. 9, can be easily analyzed in their isolated zig-zags, while the same method can hardly be adapted (without the use of a magnifying glass) to the letter of Dorothy with its density of lines, and yet the affinity between all of them can not be denied.

This incident and this comparison explain the important fact that the automatic writing of children is susceptible of an evolution, consequently every stage of the latter depends upon the corresponding age of the child between two and six years.

A new stage of graphical evolution by Aldona was apparent when she continued to produce absolutely isolated and disparate graphic specimens, which like the second letter of Dorothy show many affinities with Latin and Greek, Figs. 10 and 11 and even with oriental symbols. We perceive often on these kinds of letters W, M, N, O, B, S, frequently e and t, seldom or never k, g or d, because of their complexity of lines. Besides we find letters and symbols like the following Fig. 12.

The last stage of this evolution was a mixture of automatically produced lines and their complexes and the beginning of imitation of letters of the alphabet printed and written by adults. Aldona was then four years and eight months of age but very premature in her mental evolution.

We have thus detected five following distinct stages in the evolution of automatic writing by children.

1. (*a*) Incoherent lines produced with obvious timidity and clumsiness in moving the hand (Figs. 1, 2 and 4).

(*b*). Automatic and unattentive scribbling or chaos of straight and concentric lines, limited only by the edges of the paper, by two to two and a half years old children.

FIG. 10

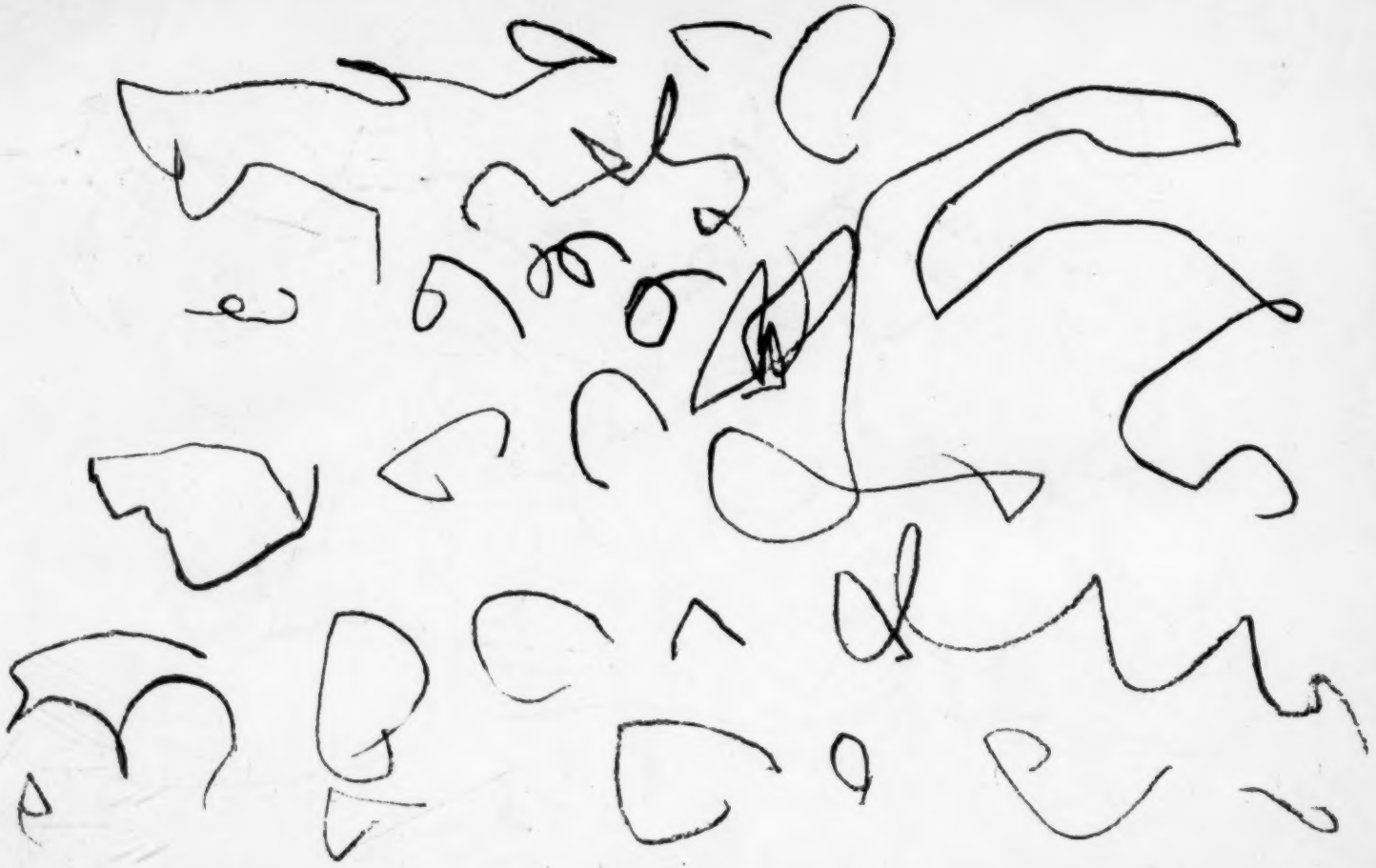

FIG. 11

2. Circular, perpendicular and horizontal waving lines with small amplitude but very long phases in two and a half to three years old children.

3. Continuous curves with high amplitudes and a notable diminution of phases, in three to five years old children with much attention brought into the writing exhibit (Figs. 5 and 13).

4. More or less isolated zig-zags with unconscious imitation of letters and symbols of the writing in various languages (Figs. 6, 8, 9 and 7, 10, 11).

5. Conscious imitation of printed or cursive writing of adults, mixed with the previous graphic elucubrations.

II

One question remains now open and that is: can these results of evolution of automatic writing observed in a few children only, be expected in every individual between the age of two and six years?

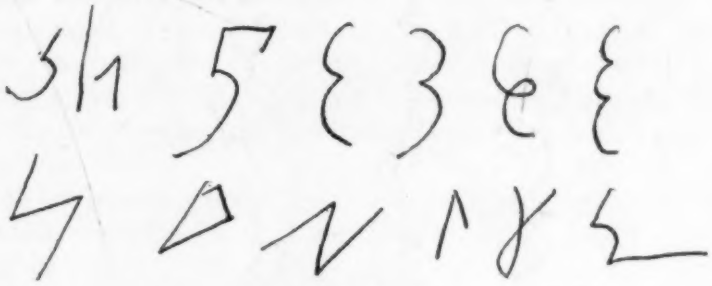

FIG. 12

In order to answer this question I submitted to special tests, shortly before the publication of this paper, many children of friends and especially children in a kindergarten school, when I was able to demonstrate all the five stages of spontaneous writing previously described (Figs. 4, 9 and 13). No child was opposed to such a test, although it sometimes happened that a child of retarded mental evolution was also retarded in its graphico-automatic development; but it never happened that a normal child three years of age should produce a writing of the first stage, or that a child two years old

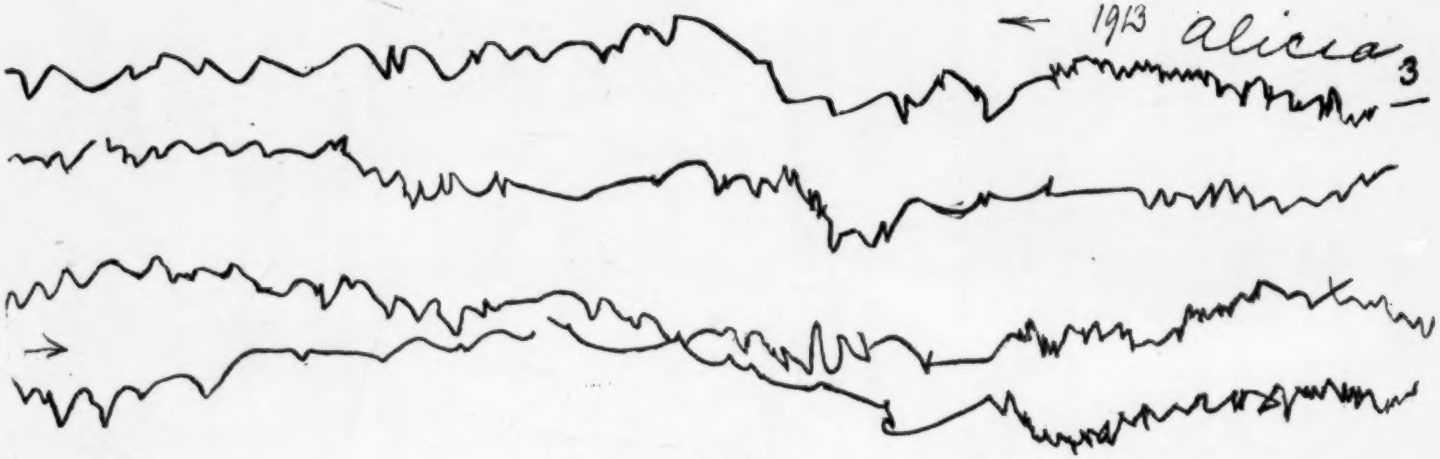

FIG. 13

should show the capacity of an older one. The smaller children will always start with incoherent lines, betraying timidity and lack of adroitness of hand movements, while a child of three to four years will display a complexity of writing, characteristic of special affinity to the writing of adults in various languages.

At the same time I was able to make some new observations. Thus, for instance, it is easy to guess the age of the child from a specimen of its writing, as it is easy to judge of its state of general development from the evolution of spontaneous writing, whether retarded or premature. When on one occasion I showed my surprise to the teacher of the kindergarten school that a girl of four years of age could only produce straight, horizontal and vertical lines, the same teacher, not aware of the above result of my investigations, informed me that the child seemed abnormal in everything else. Another observation in reference to spontaneous writing suggested to me by two little girls of the kindergarten school is that some children write spontaneously from the right side of the paper to the left using (Figs. 9 and 13) however their right hand. On my special request one of them began to write both ways and it became obvious that her writing from left to right was much slower than in the opposite direction.

In reference to psychological considerations, I observed that children from two to two and a half years of age are writing without attention, displaying simply automatic movements of the hand for the pleasure of moving; children from three to four years write with more interest and confidence of their graphical ability, while those of five to six years are more sceptical because they have some perception of the existence of the cultural writing of adults. But when requested to write whether alone or in company, they show the will to do their best and without any tendency to conscious imitation of each other. I let seven children write at the same time at one table, and I obtained specimens of different stages of evolution in spontaneous writing each corresponding to the age of the child. This opportunity to observe many children in

reference to spontaneous writing has confirmed the previous statement, that every stage of graphical evolution is connected with corresponding age in accordance to the degree of general development of the child. We have therefore the right to claim with all certainty that every child without exception is subject to a graphico-automatic evolution, which can be strictly described, and which in a certain degree helps it to the acquisition of the trained writing.

Extending our views on writing in general we can contend with equal right that graphical faculty must have been in the dim past the direct cause and source of the impulse which had for its aim the beginning of writing in general.

III

It was mentioned before that it is possible without any difficulty to detect in the third and fourth stage of automatic writing by children some letters of the alphabet of various languages, modern and ancient, which fact requires explanation. This phenomenon can not be regarded as an effect of imitation but as a result of the existence of the same graphical components in both the automatic and trained writing.

Thus if we analyze all the stages in the evolution of automatic writing of children, we find in it four or five graphical elements like the point, the horizontal and vertical, the circular or semi-circular line and especially the waving line in which the relation between the amplitude and the phase continuously changes.

Passing now to a similar analysis of cultural or trained writing, we have first to take account of two different modes in their historical evolution, from which one operate especially with disparate symbols like in oriental languages, and the second introduces the curve connecting different letters of the alphabet into words, like in modern languages. In the first category of cultural writing the graphical elements are about the same as in the automatic writing of children, namely the point or groups of them, the vertical and horizontal, circular and semicircular line; in the second group the same graphical elements are used as before, but combined and

joined with a continuous curve. The latter plays an important part thus in both the cursive and the automatic writing of children.

These complexes of lines used in cursive writing are the following: the circular line, like in "o"; the union of the latter with a part of the waving line, like in "a" and "d" or "g"; the combination of the vertical and semicircular line like in "blpcmnrpsuw" etc.; the compounds of the vertical and horizontal like in "t"; and at last the crossing of two lines, like in x and z.

We must concede now that in every language some elements or their complex prevail. Thus the circle or semicircle with a point or a group of points has the preponderance in the Arabic writing. Of 27 letters 21 are circular. In the Hebrew the combination of a vertical and horizontal line is prevalent. In 23 Hebrew letters 19 are of this kind. Sans-

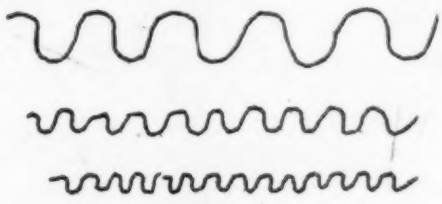

FIG. 14

crit shows a more complex structure and especially a combination of both of the former elements, prevailing in Arabic and in Hebrew. The crossing of two lines appears nearly in all of these languages although having different phonetic meaning. In Latin prevail the round, in German the vertical line and so forth.

In consideration of the fact that the continuous curve, or waving line is the most important element in both, the automatic writing of children and in writing of modern languages, we will conclude this study by referring to a detail of small importance, which however seems to be of significance for the demonstration of the origin of writing.

During my long observations of spontaneous writing by children I noticed incidentally that it is impossible for a child

as well as an adult to design the following continuous curve (Fig. 14) on a small scale without successive interruptions, which result from organic impulses. These interruptions in the curve decrease as the curve increases in size, finally disappearing; they become more and more apparent as the curve diminishes; these are like two limits between which the hand movements accommodate itself to writing.

But the important point of this view is that the above continuous curve like the various stages of automatic writing of children as well as their elements cited above, seemed to confirm the theory of organic origin of cursive as well as of all other writing. We are therefore inclined to believe, that as it is necessary, as I think, to recognize in the phonetical elements of a child speech the organic means from which human speech in general evolved, so it is equally essential to recognize in the graphical elements of the automatic writing of children with all its stages and especially in ability to produce the continuous curve, the organic basis from which the cultural writing has evolved.

VARIATIONS IN EFFICIENCY DURING THE WORKING DAY

BY H. L. HOLLINGWORTH

Columbia University, New York City

In the general literature of this topic there is a rather common failure to distinguish carefully between two questions which are really quite different from each other. There is on the one hand the question explicitly formulated by Marsh in his study of 'The Diurnal Course of Efficiency.'[1] Are there normally recurring periods of efficiency or inefficiency during the working day? Such variations, if they exist, might be due to such factors as changes in temperature, humidity, electrical conditions, meals, waking and sleeping, etc., extra-nervous changes, that is, which might supposedly be present independently of the character or degree of the activity of the nervous system. Or they might be due to essential nervous rhythms. Such influences it would be exceedingly difficult to disentangle from the many other factors bearing on momentary efficiency in a work process,—such factors as presence or absence of previous work during the day, character and amount of such work, etc. With such influences the present study is not concerned. There is, indeed, no evidence that they exist.

The other question has to do with the ability to do work continuously through long periods of time, the influence of work already done on momentary efficiency and on work yet to be accomplished. Here belong the many studies of fatigue, such as those of Cattell, Thorndike, Arai, and others.

Two things detract from the conclusiveness of most of these studies, however much the investigators may have endeavored to avoid them. The first has been the failure to reach practice limits in the work measured before beginning

[1] *Archives of Psychology*, Columbia University, No. 7.

the fatigue experiments. As a result of this failure most findings of no loss of efficiency with continued work mean merely the balance of loss and gain from two distinct sources. Findings of fatigue, similarly, have meant only a surplus of loss over gain.

The second defect has been the failure to extend the work period over a long stretch of time, or, when long periods were considered, the failure to secure intermediate measurements between beginning and end of the periods. The result of the first form of this defect is that we have but few accurate pictures of the course of efficiency through a whole working day. That of the second form is that the course is, in these studies, not shown at all.

A fortunate opportunity to study the work of a fairly large number of individuals (16) for a considerable number of working days (40) with the full time of the subjects at the disposal of the experimenter and with all the working conditions, such as daily routine, incentive, practice, etc. under unusually full control, was so planned as to afford data on this important phase of the psychology of work. The chief results bearing on this topic are here presented.

The Present Experiments

During the course of a prolonged investigation of the influence of caffein on performance in simple mental and motor tests, the schedule of doses and assignment of squads were so arranged that a study of normal performance on control days could also be made. One group of four people constituted a control squad throughout the experiment. The remaining eleven subjects were given the caffein doses according to a schedule which gave 17 coincident control days for all of the individuals. Including the corresponding days from the records of the control squad, this gave 17 normal days for each of 15 subjects. In the long run these days alternated with drug days, after the first week. The records for this week and for two preceding trials are discarded in the present study. They served to bring all individuals down to the secondary slope of the practice curve, and much to

reduce the variability of performance. This leaves 10 days for each of the 15 subjects. On two further days intensive tests were made on 10 subjects, working on an almost completed practice level.

The tests and technique employed have been fully described in an earlier monograph,[1] and no further account of them need be given here. Briefly enumerated, they are as follows:

1. *Tapping*—400 taps, maximal speed, elbow rest, stylus.
2. *Coördination*—100 strokes on the "three-hole" target.
3. *Color Naming*—Columbia color-naming chart, 100 squares.
4. *Naming Opposites*—50 words, medium difficulty, chance order.
5. *Calculation*—adding 17 mentally, 50 two-place numbers, chance order.
6. *Steadiness*—holding at arm's length, for one minute, a brass rod 2.5 mm. in diameter, in a hole 4.5 mm. in diameter, with as few contacts as possible.
7. *Discrimination Reaction*—reacting with right hand to red and with left hand to blue, time measured in sigma.

Such conditions of work as incentive, exercise, rest, temperature of the room, personality of the operator, etc., were fairly constant throughout the experiment, and during the latter part, on the two intensive days, conditions were still more rigorously controlled. All individuals ate at the same table, spent the whole day under the eyes of the experimenter, etc. The quality and quantity of the work, in each test, remained constant, the records being made in time of performance. The subjects were college students, graduate students, and the wives of some of the latter. All received compensation for their work, were under oath to do their best at each trial, were zealous to an unanticipated degree, and were stimulated to further rivalry by the award of prizes for 1st and 2nd places in each test. No records were disclosed during the experiment, except that after each week the names of the best three persons in each test were posted.

[1] 'The Influence of Caffein,' *Arch. of Psychol.*, Columbia Contributions, 1912, 166 pp.

To secure greater interest and effort for experimental purposes than was displayed by these individuals seems to the writer to be well nigh impossible. These individuals were not "experimenting" in the usual laboratory sense. They were regular employees, working for wage, at real work, and outside candidates were waiting to take the places of any whose work seemed unsatisfactory. All in all the experiment constituted a fairly accurate duplication of the conditions of actual work. The chief differences were that in this case the work was standardized, the conditions were controlled, and the workers were directly and intelligently interested in the method and outcome of their work.

During the preliminary part of the experiment five trials were made daily, each sitting requiring about 45 minutes. The test hours were 7.45 A.M., 10:00 A.M., 12:15 P.M., 3:10 P.M., and 5:30 P.M. During the second part of the experiment the subjects arrived at the laboratory at 10:00 A.M. and worked continuously for about 12 hours, except for two 45-minute periods for lunch and dinner. One or two of the tests were omitted in this section, and only 10 individuals gave records which could be used for the present purpose. Two control days during this section gave 15 trials each in all of the tests. This gives then 20 records (two trials for each of 10 individuals) for each test hour, the working day extending from 10:00 A.M. to 10:00 P.M.

Another individual did not take part in these tests, but worked under the same conditions and during the same periods, at typewriting. The results from this subject are discussed in a separate section of this report.

Preliminary Experiments

In this experiment there were 10 experimental control days for each of 15 subjects. These subjects are here separated into three different groups. Group I. consists of the five women; Group II. of five men whose attendance was perfect throughout both the preliminary and the intensive experiment; and Group III. of five men whose records have been grouped together because of irregularities of one kind

or another during the investigation as a whole. One subject was ill part of the time, another was compelled to leave the city on one or more days, another did not participate in the intensive experiment, another was dismissed because of

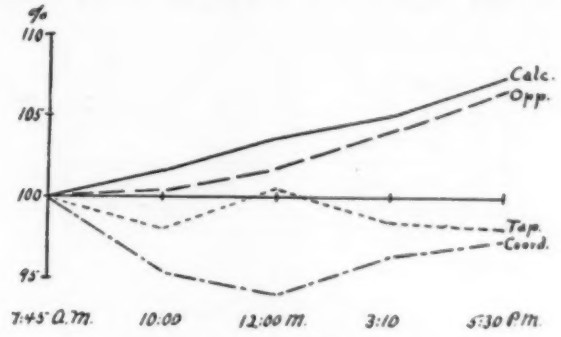

FIG. 1. Group 1, five women.

failure to comply with all the instructions while in the laboratory, and the fifth was regular but was included in this group so as to give five subjects in each of the three groups.

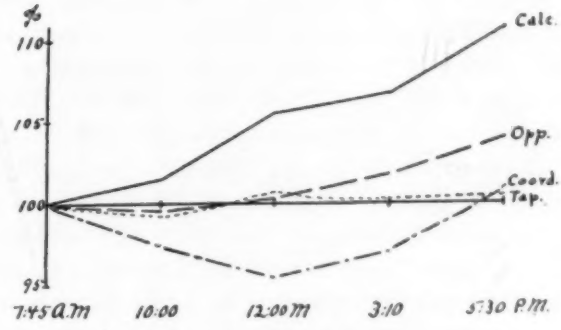

FIG. 2. Group 2, five men.

Since the members of the groups were not chosen because of their records in the tests but on these purely incidental grounds, the performance of one group will well serve to check up the performance of other groups. As a matter of fact, all three groups show the same results. The individual records are not given in this preliminary experiment, because more detailed records and probable errors are given in full

for the intensive section. The preliminary results are given in the form of curves for each of four tests and for each of the three groups. Each point in the curve is the average of 50 separate determinations, 10 records by each of 5 subjects. This gives high reliability to the course of the curves.

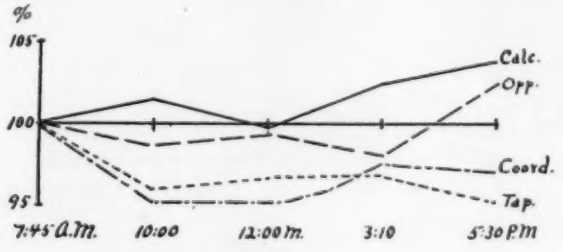

FIG. 3. Group 3, five men.

The initial performance (at the 7:30 A.M. period) is taken as the unit of measurement. Each separate record for each subject is then expressed in terms of per cent. of the morning record. This makes all records roughly comparable in spite of the differences in absolute time of performance in different tests or by different individuals.

Plate I. gives the results for the group of five women. *Calculation* and *Opposites* both show increasing fatigue at successive trials. In *Calculation* the decrease in efficiency is regular, being about 2 per cent. at each trial, giving 7.5 per cent. fatigue at the end of the day. In *Opposites* the second trial shows little inferiority, the third trial shows a falling off of 1.5 per cent., and the 4th and 5th trials each give an additional decrease of 2.5 per cent. below the preceding trial, the total thus being 6.5 per cent. by the end of the day. The curve for *Tapping* is more irregular, but its general tendency indicates increased speed rather than slowness. Only the mid-day record is slower than the initial time (.5 per cent.), all other records are 1.5 to 2.0 per cent. faster than the initial time, and the fastest record is made at the end of the day. The *Coördination* test shows increasing efficiency up to mid-day, then a falling off from this maximum, but the falling off, by the end of the day, is several per cent. less than

the previous gain. All subsequent records are better than the initial trial, the maximum efficiency coming in the middle of the day. The gain by mid-day is some 6.0 per cent. over the initial time.

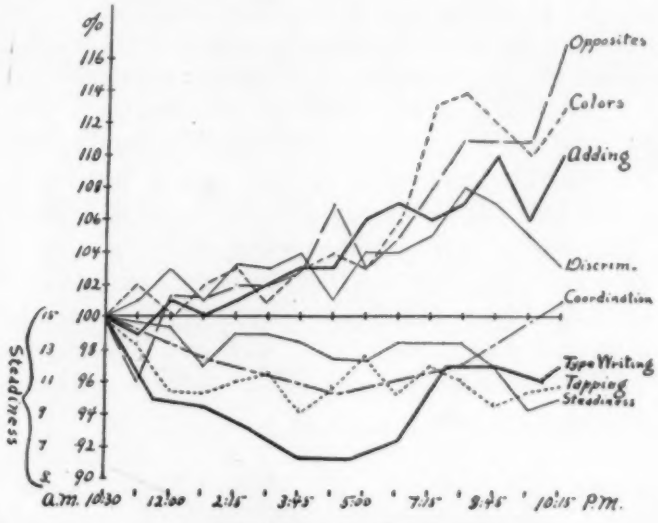

PLATE IV. Intensive Experiment.

Showing the effect of continued work on the different tests. Each record is the average of 20 determinations, 10 observers, 2 days each. Records are given in *per cent.* of initial performance, except in the case of *steadiness*, where the number of contacts is given.

Plate II. gives the records made by Group II. The results are very similar to those yielded by Group I. The fatigue in *Calculation* amounts to 10.5 per cent. by the end of the day, and is regular in its appearance from trial to trial. *Opposites* shows a smaller loss (4.0 per cent.) and most of this loss comes, as was the case with Group I., in the 4th and 5th periods. *Tapping* shows no increased efficiency with this group, but neither is there any fatigue present, the records yielding practically a horizontal line. *Coördination* shows, as with Group I., an increase of several per cent. (4.5 per cent.) by the mid-day period, and a subsequent loss, which, in this case is just equal to the previous gain.

The curves for the irregular group (Plate III.) are, as is to be expected, irregular, but their general tendency quite

agrees with the results from the two other groups. *Calculation* and *Opposites* show inefficiency by the end of the day, and the loss in the former is greater than in the latter. *Tapping* shows improvement rather than loss, and the best record is made at the last trial. The initial trial is by far the slowest of the five records. *Coördination* shows the now familiar increase by mid-day, with subsequent loss; as with Group I., the loss is smaller than the previous gain.

In general then the result of the preliminary experiment is as follows: The two more strictly mental tests show decreasing efficiency as the result of continued work through the day. The more strictly motor tests show increase rather than decrease. In *Tapping* this increase is most noticeable at the end of the day; in *Coördination* the period of maximal efficiency is greatest at mid-day. A closer and more detailed study of these relations requires the intensive experiment, the results of which follow.

The Intensive Experiment

Table I. gives a summary of the results in this section of the investigation. There were two days of work for each of 10 individuals. Each average is thus made on the basis of 20 determinations. The table gives the average time of performance in each test at each of the 15 trials between 10:30 A.M. and 10:30 P.M. The averages are expressed first in terms of absolute time (seconds). In this case the probable error of the divergence of the true from the obtained average is given at each point. By this time the individuals were all working on a practical practice level in all tests, and the probable errors are very small. The averages are also given in terms of the initial trial as unity, as in the preliminary section. The initial record thus becomes 100 per cent. and subsequent records are expressed in per cent. of this initial performance.

Six tests are included in this section. *Color Naming* and *Discrimination-reaction Time* and *Steadiness* are added to the tests used in the preliminary section, and Coördination is omitted.

TABLE I

AVERAGES FOR THE TWO INTENSIVE DAYS, 15 TRIALS EACH DAY.
Ten Individuals, Two Days Each
Table gives averages and P.E. (t–o) for all tests*

Time		A. M.		M.	P. M.										
		10:30	11:15	12:00	1:30	2:15	3:00	3:45	4:15	5:00	5:45	7:15	8:00	8:45	9:30	10:15
Tapping	Av....	49.8	49.2	47.8	47.7	48.0	48.3	47.2	47.9	48.9	47.7	48.5	48.1	47.4	47.7	47.9
	P.E....	.9	1.1	1.2	1.3	1.3	1.4	1.3	1.5	1.1	1.3	1.4	1.4	1.5	1.4	1.5
	%....	100	98.4	95.6	95.4	96.0	96.6	94.4	95.8	97.8	95.4	97.0	96.2	94.8	95.4	95.8
Color Naming	Av....	52.1	52.9	51.8	52.9	53.5	52.7	53.8	53.9	53.8	55.4	59.1	59.3	58.2	57.0	59.0
	P.E....	1.6	1.7	1.6	1.4	1.7	1.8	1.8	1.9	2.0	2.2	2.6	2.3	2.0	2.2	2.1
	%....	100	102	100	102	103	101	103	104	103	106	113	114	112	110	113
Opposites	Av....	34.3	33.0	34.5	34.7	34.8	35.1	35.5	36.6	35.3	35.9	37.1	38.2	38.1	38.0	40.1
	P.E....	1.2	1.1	1.5	1.2	1.1	1.4	1.4	1.5	1.4	1.2	1.5	1.5	1.6	1.6	1.7
	%....	100	96	101	101	102	102	103	107	103	105	108	111	111	111	117
Adding	Av....	86.1	84.8	86.9	85.8	86.6	87.9	88.5	88.5	91.1	92.3	91.5	91.8	94.9	91.5	94.5
	P.E....	2.1	2.8	2.5	2.3	2.2	2.5	2.8	2.5	2.3	2.6	2.5	2.8	2.8	2.9	3.0
	%....	100	99	101	100	101	102	103	103	106	107	106	107	110	106	110
Discrimination	Av....	328	331	338	331	339	338	342	333	342	343	344	355	351	344	338
	P.E....	6.4	7.2	7.4	7.4	8.4	7.2	7.4	7.4	7.6	8.8	7.2	8.2	8.2	7.4	6.8
	%....	100	101	103	101	103	103	104	101	104	104	105	108	107	105	103
Steadiness	Av....	15.0	15.0	14.6	12.0	14.2	14.2	13.8	12.7	12.5	13.5	19.1	13.6	11.9	9.6	10.1
	P.E....	3.5	2.6	3.6	2.5	3.5	3.9	3.6	4.5	2.6	3.8	3.5	3.0	3.8	2.2	3.0
	%....	100	100	97	80	94	94	92	85	83	90	126	91	79	64	65

* P.E. (t–o) = $\frac{.84435 \text{ A.D. dist.}}{\sqrt{n}}$.

This form of P.E. means that in several such experiments the various divergences of the obtained averages from the true average would be distributed about o as a mode, with the figure given as the P.E. of the distribution in question.

The measures, in terms of per cent. of initial performance, are also expressed graphically in Figure IV. Because of the great range of change in the *Steadiness* test (from 100 per cent. to 64 per cent. at the end of the day) the records for this test are not expressed in per cent., but in number of contacts made.

The difference between the mental tests and the motor tests, already pointed out in the preliminary section, is at once apparent in this intensive experiment. The four mental tests show pronounced decrease in efficiency as the day progresses. The motor tests show just the reverse tendency,—an increase in speed of tapping and an increase in steadiness. The eleventh trial in *Steadiness* shows a large increase in number of contacts, but this was the test just after the individuals had come into the laboratory after the evening meal, with its attendant general activity and excitement. This record is given in the table, but omitted from the curve. The *Steadiness* curve, when thus drawn, shows a clear increase in steadiness by the end of the work period. The curve for *Tapping* shows no such uniform increase, but the first two trials are the slowest of the 15, and the four trials at the end of the day are among the best of the 15.

Performance in the four mental tests becomes less efficient at a fairly uniform rate, up to about the 10th trial, beyond which point the separate curves begin to diverge. *Opposites* and *Color Naming* show, at the end of the day, the greatest amount of loss. *Calculation* shows a somewhat smaller loss than these two tests. *Discrimination* ceases to show loss after the 12th trial, the next three trials showing an increasing efficiency instead. The last trial of the day, in *Discrimination* is thus about equal to the average record at the middle of the day. It is to be noticed that in only two cases, in these mental tests, is the initial record ever surpassed at later trials,—namely at the second trial in *Adding* and *Opposites*. In these tests the initial work seems to have served as a warming up process, which facilitated somewhat the next trial. But beyond the second trial the initial records are never excelled. This is in striking contrast with the motor tests, in which the initial trial was, without exception, the least efficient trial in the whole day's work.

It should be noted that during these intensive days there was practically no improvement in any of the tests, either during the day or from day to day. *Adding* and *Opposites* are the only tests which show any such gain at all, and here

the gain from one day to the next is all that shows and this amounts to only 1 per cent. and 3 per cent.

Typewriting

One individual, a woman of 38 years, already proficient in typewriting by the touch method, worked at typewriting instead of at the tests just discussed. Ruskin's 'Sesame and Lilies' was chosen as material to be copied. Each page contained 27 lines, the lines averaging 35 characters each. The subject kept her own time records, and corrected all mistakes noticed, at the time they were made. During the intensive experiment the subject came to the laboratory daily at 10:00 A.M. and wrote four pages each hour, up to 9:15 P.M. (excepting short intermissions for lunch and dinner). This made 10 trials each day. Two of the three days are used in this connection, one control day and one day on which the caffein amount was such that no drug influence was present. The times, for each hour, in minutes, are as follows.

Table II

Typewriting Records

	Hours of the Day										
	A. M.		P. M.								
	10	11	1	2	3	4	5	6	8	9	
1.....	30.0	29.6	29.6	27.8	28.1	28.3	27.8	29.6	28.8	29.0	Time.
	66	65	60	48	52	45	43	52	34	38	Errors.
2.....	31.0	28.5	28.1	29.0	27.5	27.3	28.5	29.8	30.6	29.6	Time.
	68	36	55	65	34	34	43	53	55	60	Errors.
Total.....	61.0	58.1	57.7	56.8	55.6	55.6	56.3	59.4	59.4	58.6	Time.
	134	101	115	113	86	79	86	102	89	98	Errors.

Although the figures are few, they are fairly reliable, since the work period measured at each trial was half an hour of continuous work, thus allowing abundant time for compensation of incidental variations. On both days the time of performance decreases to a point of maximum speed at 3:00 and 4:00 o'clock. Beyond this point speed is reduced until by 9:00 o'clock the same time is required as at the beginning of the day's work. When the results for the two

days are added together, this result is very clearly shown. There is constant increase in speed up to 3:00 and 4:00, then gradual loss again. This increase in speed is not gained at the expense of accuracy. Quite the contrary, the hours of fastest speed are also the hours at which the fewest errors are made. The numbers of errors show the same tendency to reduction at this period of maximum efficiency, with increase on either side. The net result of the experiment on typewriting is then quite like that found in the simpler coördination test, in the preliminary experiment, except that the maximum efficiency comes at a somewhat later point in the former than in the latter. A morning inertness with a mid-day gain and an evening loss which is slightly less than this gain, is the general rule for both tests. The same rule holds for accuracy as for speed.

These coördination tests doubtless stand more or less on the borderline between the mental and motor performances, since they involve a considerable amount of perception, discrimination and control, although depending, in their actual performance, largely on the actual motor speed which is possible. The trough-like curve shown by the coördination tests is apparently a combination of the effects on the two extreme types of process. During the middle of the day the increasing motor speed is sufficient to overcome the mental fatigue which comes fairly slowly for the first few hours. After that point the mental fatigue increases so rapidly as to quite offset the effect of the increased motor speed which is still present.

Discussion

It is interesting to compare these results with those of Cattell, Thorndike, Arai, and others who have made extended practice and fatigue curves extending over long work periods, and with the results of Marsh and others who have sought for evidence of normally recurring variations of ability at different periods of the working day.

Only a few of Marsh's records have significance in themselves. The results are complicated by practice, which was

often allowed for on the basis of rather arbitrary assumptions. There were also only a few subjects who worked in any intensive way, and then usually for not over two weeks. But the results which are consistent and reliable in Marsh's work agree fairly closely with those of the present study.

The tests for speed and coördination of movement (striking squares, snapping, three-hole test, tapping) were similar in character to those here employed and the results are quite similar. "Most worthy of note . . . is the exceptional occurrence and decisiveness of the maximum tapping rate at 9:00–10:00 P.M. . . . . The maximum of accuracy comes earlier in the day than the maximum for speed . . . there is no doubt that it falls somewhere in the middle portion of the day" (p. 21). These tendencies are quite confirmed by the present results from *Tapping*, *Coördination* and *Typewriting*, in which mere speed showed a maximum at the end of the day, and speed with accuracy prescribed (*Coördination*) at the middle of the day.

In the case of strength Marsh found (using Cattell's ergograph and Collin's dynamometer) that "on the whole both subjects show most strength in the middle period (3:30 to 4:30 P.M. . . . . The curve of strength efficiency seems well established . . . a beginning minimum in early morning, a fairly rapid rise till 11:00, a level or slight decline till 1:00 P.M. (1 hr.), an increase to the maximum at 5:00 (1 hr.), thence a fall till bed time. . . . As a whole the figures show the same general course of efficiency for both sides of the body" (p. 31). These results are confirmed by work by Lombard, Patrizi, Harley, Christopher, Smedley, Oseretzkowsky and Kraepelin, and Storey.

In the cases of reaction-time, color naming, and opposites, in Marsh's work, the tests were so fragmentary and the subjects so few that the occasional superiority of successive trials is almost certainly only the result of practice, adaptation to the work, etc. Marsh remarks that in reaction time the superiority of the noon and late afternoon periods is apparent. In his simple sound-reactions this is the case, but in the discrimination reactions the superiority is anything but apparent.

In the case of memory Marsh's only reliable figures are those from one individual who memorized lists of German words at intervals of 1.5 hours, from 7:00 A.M. to 10:30 P.M. This is similar to the work of my own subjects on the intensive days, for the whole day (for 12 days) is said to have been spent in experimentation. Marsh was looking for variations within the day, and concludes (p. 53), "the total outcome as to memory must be considered a negation of the existence of a simple diurnal memory curve." As a matter of fact the records give a curve quite like that of all the mental tests in my own experiment, with clear signs of decreasing efficiency as the day goes on. The averages are as follows:

Hour	Time Taken
7:00 A.M.	371 sec.
8:30	390
10:00	386
11:30	434
1:30 P.M.	475
3:00	460
4:30	445
6:00	431
7:30	444
9:00	544
10:30	613

Many years ago Cattell studied the influence of fatigue on various form of reaction time by means of the records made by two observers who worked continuously for many hours. He wrote as follows: "In order further to investigate the effects of fatigue, I made extended series of experiments in which 1,950 reactions were made in succession, the observer reacting continuously from early in the morning until late into the night. Three series (78 reactions) were made with light, then three series (39 determinations but 78 mental processes) in which white light was distinguished and reacted on, then three series in which letters were seen and named, then two series in which associations were made, lastly three series of reactions on sound. This entire combination was repeated six times. The experiments were begun both days at 7:30 A.M. and were concluded in the case of *C* at 1:30 A.M. and in the case of *B* at 11:00 P.M., short pauses being made

for meals. One series of each variety was made the following morning and evening; in the case of *C* a further set of series the day after. . . . The first result to be noted from the table is the very slight effects of fatigue; in no case is the time lengthened more than a couple of hundredths of a second, and the mean variation is but little increased."[1]

Of the tests used by Cattell two were quite similar to tests used in the present experiment, viz., the discrimination reaction and the naming test (letters). His results for these two tests are as follows:

Time	Discrimination Reaction				Naming Letters			
	Obs. *B*		Obs. *C*		Obs. *B*		Obs. *C*	
	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
7:30 A.M.	198	21	247	12	344	25	439	23
9:40	193	17	230	16	354	27	431	19
1:00 P.M.	207	17	248	21	337	34	448	23
2:50	207	25	237	19	348	26	458	20
6:55	232	18	253	21	356	31	476	26
8:50	218	19	249	16	381	30	469	23

In discrimination the fatigue by the end of the day (comparing last three trials with first three) is about 14 per cent. and 3 per cent., and in naming letters 7 per cent. and 8 per cent. with the two observers, about the same decrease in efficiency as is shown in these two tests in the present experiment, after the same amount of work.

In a recent study[2] Arai has presented an interesting picture of the course of efficiency during a day of continuous work. After considerable preliminary practice she reached an approximate practice level of performance in multiplying four-place numbers by four-place numbers, mentally. She then worked continuously (except for meal times) for 12 hours on each of four successive days, at such multiplication. During these four days there was, to be sure, increased speed due to practice, but this amounted to only about 20 per cent. of the initial speed by the end of the fourth day, thus averaging but 5 per cent. during any one day. The records are from

[1] *Mind*, 11, 1886, p. 537.

[2] 'Mental Fatigue,' T. C. Publications, 1912.

but one observer, but apparently from one who worked with unusual zeal and faithfulness. The curves of daily work, for each day, show pronounced and fairly regular diminution of efficiency, as the result of continued work. "Difficult and disagreeable continued work brings about a decrease in the efficiency of the function exercised. In the case of T.A. the time taken to do a certain number of examples is almost doubled during twelve hours of mental multiplication" (p. 114).

This is a much greater loss of efficiency than is shown in the present experiment, and the reasons are of course obvious. The mental multiplication of pairs of four-place numbers is a much more strenuous task than any of the tests applied to the present group of subjects. Further, in the present instance, the task was varied from time to time as the individual went through the various tests in succession. It is further true that these individuals travelled through the tests in squads of three each, so that after each test, each observer had a brief moment of rest while the other two made their records, and while the whole squad passed to an adjoining room for the next test.

Marsh insists strongly on the presence of a morning *inertness* which is the result of understimulation and protracted drowsiness. No such inertness is shown in my results, except in the motor tests. The inertness is probably not so much a nervous fact as it is a muscular mechanical one. In fact, as Marsh points out, "It is pronouncedly manifest in muscular abilities, especially strength, but seems less and less apparent and persistent as the mental field is entered."

The sharp difference between the effect of continued work on motor abilities, such as strength, speed, and coördination, and the effect on mental tests of perception, association, discrimination, memory, etc., is interesting. It undoubtedly falls in line with several other suggestions concerning the relations between the two kinds of work, and thereby throws considerable light on the general mechanisms of work. In the present investigation, as the day's work proceeds, motor processes gain and mental processes lose in

efficiency. Is there any significant relation between these two tendencies? The relation recalls Rivers's remark that he finds in himself and in others greater muscular power in moments of great mental fatigue or weariness. He concludes that the output of motor energy is ordinarily under the control of some inhibitory mechanism in the higher brain centers. During fatigue, after taking doses of certain drugs, etc., this inhibition seems to be weakened and there is for the time being an unrestrained overflow of motor innervation which quite exceeds the normal. Marsh also remarks (p. 79): "Where my subjects noted mental depression or even headache on their records, the figures rarely failed to show a high grade of muscular performance at the time."

The facts also fall well in line with Thorndike's suggestive remark that "we are fatigued not so much by what we do as by what we do not do." We may assume, rather roughly, perhaps, yet not without meaning, that as mental work proceeds, motor impulses and innervation tendencies which are of necessity suppressed during such work, become more and more insistent, and, according to the degree of insistence, interfere with the cortical interplay involved in the mental work. We would then have just the obverse of Rivers's explanation, for much the same set of facts. Motor processes would become more vigorous, not because of the removal of inhibitions, but, becoming more and more vigorous by cumulative impulsive tendencies, would themselves bring about the inhibitions which show themselves by way of diminished mental work.

Marsh remarks (p. 89): "The extent to which the inefficiency of the later part of the day is due to weariness and how far to real fatigue and nervousness is a hard question to solve." Other writers have also frequently suggested that the ordinary fatigue curves secured in experiments or incidentally shown in practical work, in no sense reveal an actual diminution of organic efficiency, but picture rather the effects of monotony, ennui, loss of interest, work, habits, etc. The great importance of these factors, both in the laboratory and in daily life, must of course be recognized. But it does

not at once follow that there is no genuine change in the nervous mechanism underlying the distribution and application of energy, as the result of continued work.

In the first place we should probably expect these general factors of weariness, etc., to affect all the tests in much the same way. There is at least no obvious reason why loss of interest should increase efficiency in *Tapping*, *Coördination*, and *Typewriting*, during the same periods in which it produces inefficiency in the whole range of more strictly mental performances. Nor is there obvious reason for believing that one test gains while another loses in interest. In fact all the obvious reasons are to the contrary. Nevertheless the various tests show characteristic differences in the effect, upon them, of continued work.

Further evidence that the loss of efficiency here portrayed is not due to such by-products as weariness, monotony, loss of interest, etc. is clearly shown, in the original experiments, by the influence of caffein on the drug days. The speed of performance, in *Opposites*, *Color Naming*, and *Adding*, for example, after the loss of efficiency has occurred, is strikingly quickened by appropriate amounts of the drug. It is not easy to see how the presence of caffein in the system modifies the interest of the tests (the change is not present after control doses).

That the form of the curves is not due to organic rhythms or diurnal factors is shown by the fact that the same curves are secured whether the work begins at 7:30, as in the preliminary experiment, or at 10:30 as in the case of the intensive experiment.

Most experimental attempts to demonstrate the presence or absence of genuine fatigue during the working day are invalidated because of the presence of improvement, either during the day or from day to day. In discussing his experiments on mental fatigue, Thorndike points out that incompetency does not come in proportion to the work done. "The decrease in energy does not have enough influence to outweigh the influence of practice."[1] This shows the neces-

[1] Psych. Rev., 7, 1900, 466, 547.

sity of using as subjects in fatigue experiments individuals who have already reached a practice limit in the tests used. And the limits should be genuine,—of such a sort that not only is there no gain during a given work period, but also no improvement as between one work period and the following one. This is indeed a difficult thing to accomplish with any great number of subjects, and in this respect the present experiments are particularly fortunate. This fact, coupled with the unusual zeal and constancy of incentive secured, lend high reliability to the data.

The conclusions, it should be noted, do not point to a general or special *fatigue factor*, nor to periodic variations of efficiency within the working day (except in coördination). They point rather to the presence of a complex work mechanism of such a sort that the influence of continued work upon more strictly mental processes differs characteristically from the effect on processes essentially muscular in character. This mechanism is one which stands over and above the transient inhibitions and reinforcements conditioned by interest, incentive, weariness, and similar affective or volitional factors.

Whatever the nature of this mechanism may be, it is such that processes essentially motor in character are facilitated and quickened by continuous work; processes involving coördination are first accelerated and then retarded again, approaching their original speed; processes essentially mental in character, when the work is done on a practice level of efficiency, show fairly uniform loss of efficiency, amounting to from 10 to 15 per cent., by the end of the day. Furthermore, the mechanism is of such a nature that it is acted upon in very definite ways, by the application of various drugs.

DISCUSSION

THE INHIBITORY FACTOR IN VOLUNTARY MOVEMENT. REPLY.

In the September number of the REVIEW Professor George V. N. Dearborn has discussed my article on "Voluntary Movement under Positive and Negative Instruction." His last paragraph begins: "None the less, it is interesting, to me at least, to observe that of Langfeld's five subjects that one (D) who had consistent kinesthetic imagery made the best record, a fact Langfeld fails to note in his conclusions, although obvious in the protocols." Since Professor Dearborn believes that "we are wasting not a little time and query over the matter of imagery in studying voluntary movement" the only interest he can have in this discovery is that it portrays extreme stupidity or intentional neglect upon my part. He has been, however, somewhat hasty in his criticism. I could not expect that he should be familiar with the Whipple tracing-board or that he could see from the rather indistinct lettering in the illustration of the board in Whipple's Manual, p. 120, that the larger numbers are at the wider end and that the stylus approaches zero as it moves down the groove. If, however, he had examined the tables and the protocols and the manner in which I treated the results, he would have discovered that the larger the figures the worse the results and not the reverse, as he has carelessly assumed. It is clear that subject D made the worst record.

H. S. LANGFELD.

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